LIBRARY 18

JUN 3 193

Vol. XVI, No. 3 *PSYCHOLOGICAL REVIEW PUBLICATIONS* JUNE, 1933

Journal of Experimental Psychology

EDITED BY

SAMUEL W. FERNBERGER, UNIVERSITY OF PENNSYLVANIA

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
WALTER S. HUNTER, CLARK UNIVERSITY (*Index*)
HERBERT S. LANGFELD, PRINCETON UNIVERSITY (*Monographs*)
EDWARD S. ROBINSON, YALE UNIVERSITY (*Bulletin*)

CONTENTS

Conditions of Transfer of Training: R. W. BRUCE 343

The Effect of the Initial Chances for Right Responses upon the Efficacy of Intensified Reward and of Intensified Punishment: I. LORGE 362

The Comparative Strengthening of a Connection by One or More Occurrences of It in Cases Where the Connection was Punished and was Neither Punished nor Rewarded: I. LORGE 374

The Increase of Visual Acuity in One Eye Through Illumination of the Other: G. W. HARTMANN 383

Changes in Visual Acuity Through Simultaneous Stimulation of Other Sense Organs: G. W. HARTMANN 393

Intensity of Light and Area of Illuminated Field as Interacting Factors in Size of Pupil: C. E. FERREE, G. RAND AND E. T. HARRIS 408

Runway Time and the Goal Gradient: A. C. ANDERSON 423

The Relative Retention Values of Stylus and Mental Maze Habits: R. H. WATERS AND G. B. POOLE 429

The Relation between Two Methods of Learning Piano Music: R. W. BROWN 435

Learning as a Function of Water Temperature: E. R. HACK 442

A Technique to Control, and to Measure the Effects of Fixation in the 'Range of Attention' Experiment: H. R. CROSLAND 446

An Improved Technique in the Mirror-Tracing Experiment: C. E. LAUTERBACH 451

Discussion: Memory for Pleasant, Unpleasant, and Indifferent Pairs of Words: P. T. YOUNG, 454. *Reply: Methods of Studying the Learning and Retention of Pleasant and Unpleasant Activities:* H. CASON, 455. *A Note on Tolman's 'Disproof' of Thorndike's Law of Effect:* F. L. GOODENOUGH, 459. *Reply: The Law of Effect: A Reply to Dr. Goodenough:* E. C. TOLMAN, 463.

PUBLISHED BI-MONTHLY
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION
BY THE PSYCHOLOGICAL REVIEW COMPANY
PRINCE AND LEMON STS., LANCASTER, PA.
AND PRINCETON, N. J.

Entered as second-class matter, July 1, 1920, at the post-office at Lancaster, Pa.

PUBLICATIONS

OF THE

AMERICAN PSYCHOLOGICAL ASSOCIATION

EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
S. W. FERNBERGER, UNIVERSITY OF PENNSYLVANIA (*J. Exper. Psych.*)
WALTER S. HUNTER, CLARK UNIVERSITY (*Index and Abstracts*)
HENRY T. MOORE, SKIDMORE COLLEGE (*J. Abn. and Soc. Psychol.*)
HERBERT S. LANGFELD, PRINCETON UNIVERSITY (*Monographs*)
EDWARD S. ROBINSON, YALE UNIVERSITY (*Bulletin*)

HERBERT S. LANGFELD, Business Editor

PSYCHOLOGICAL REVIEW

containing original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 540 pages.

PSYCHOLOGICAL BULLETIN

containing critical reviews of books and articles, psychological news and notes, university notices, and announcements, appears monthly (10 numbers), the annual volume comprising about 720 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

containing original contributions of an experimental character, appears bi-monthly, February, April, June, August, October, and December, the six numbers comprising a volume of about 900 pages (from Jan. 1, 1933).

PSYCHOLOGICAL INDEX

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The INDEX is issued annually in June, and may be subscribed for in connection with the periodicals above, or purchased separately.

PSYCHOLOGICAL ABSTRACTS

appears monthly, the twelve numbers and an index supplement making a volume of about 700 pages. The journal is devoted to the publication of non-critical abstracts of the world's literature in psychology and closely related subjects.

PSYCHOLOGICAL MONOGRAPHS

consists of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. The MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages.

JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY

appears quarterly, April, July, October, January, the four numbers comprising a volume of 448 pages. The journal contains original contributions in the field of abnormal and social psychology, reviews, notes and news.

ANNUAL SUBSCRIPTION RATES

Review: $5.50 (Foreign, $5.75). **Index:** $4.00 per volume.
Journal: $7.00 (Foreign, $7.25). **Monographs:** $6.00 per volume (Foreign, $6.30).
Bulletin: $6.00 (Foreign, $6.25). **Abstracts:** $6.00 (Foreign, $6.25).
Abnormal and Social: $5.00 (Foreign, $5.25). Single copies $1.50.
Current numbers: **Journal,** $1.25; **Review,** $1.00; **Abstracts,** 75c; **Bulletin,** 60c.

COMBINATION RATES

Review and Bulletin: $10.00 (Foreign, $10.50).
Review and J. Exp.: $11.00 (Foreign, $11.50).
Bulletin and J. Exp.: $12.00 (Foreign, $12.50).
Review, Bulletin, and J. Exp.: $16.00 (Foreign, $16.75).
Review, Bulletin, J. Exp., and Index: $19.00 (Foreign, $19.75).

Subscriptions, orders, and business communications should be sent to the

PSYCHOLOGICAL REVIEW COMPANY

PRINCETON, NEW JERSEY

Journal of
Experimental Psychology

VOL. XVI, No. 3 JUNE, 1933

CONDITIONS OF TRANSFER OF TRAINING

BY ROBERT WALLACE BRUCE[1]

Wabash College

INTRODUCTION

This study is an investigation of some of the conditions of transfer of training. We shall seek to answer these questions:

I. What is the relation of the following conditions of learning to transfer:

A. Learning to make a new response to an old stimulus?

B. Learning to make an old response to a new stimulus?

C. Learning to make a new response to a new stimulus?

II. What is the relation of the degree of integration of the initial learning to transfer in

A. Learning to make a new response to an old stimulus?

B. Learning to make an old response to a new stimulus?

III. What is the relation of certain similarities between the $S_1R_1S_2R_2$ terms of the initial and subsequent learning material to transfer in

A. Learning to make a new response to an old stimulus?

B. Learning to make an old response to a new stimulus?

In most of the previous studies of learning transfer, the stimulus-response terms are undifferentiated. The usual procedure is to learn task *A*, then task *B*, check and compare the

[1] From the Psychological Laboratory of the University of Chicago. The writer wishes to acknowledge his indebtedness to Professors Edward S. Robinson and Arthur G. Bills for assistance in planning and completing the study.

two results, and ascribe the difference to transfer. As a consequence, results have been obtained which give positive, zero, and negative transfer. Since the previous experiments have not been made for the purpose of reconciling these diverse results, we are attempting such a reconciliation by discovering some of the conditions of transfer.

All transfer studies may be classified on the basis of whether or not a differentiation is made between the stimulus and response terms of the experimental situation, and the extent to which these terms are subjected to variation. According to this classification, transfer studies may be grouped into three classes.

I

In the first class of studies, *the stimulus-response terms in the experimental situation are undifferentiated.*

The great mass of transfer studies belong to this class. Examples include the studies of Wiltbank, Webb, Ho, Poffenberger, Waters, Kline and Owens, Swift, Starch, James, Scripture Smith and Brown, Thorndike and Woodworth, Winch, Ebert and Neumann, Bogardus and Henke, Fraker, Bagley, Judd, Richardson, and many others.

II

In the second class of studies, *the stimulus-response terms in the experimental situation are differentiated, and the influence of one variable is examined.*

The studies of Hunter, Pearce, Wylie, Bair, and Kline belong to this class.

Hunter, using a T-shaped discrimination box, teaches rats to turn to the right when given a light and to the left when not given a light, then to the left when given the light and to the right when not given the light; the rats must thus learn to make a new response to an old stimulus. The results are given in average total number of trials required in learning per habit.

Habit I, 286
Habit II, 603
Negative transfer, 111 percent

Pearce obtains approximately the same results in an earlier study.

Kline teaches subjects to respond to given pieces of literature, such as the Scarlet Letter, with the name of some author other than the correct one; moreover the subjects are classified in respect to their familiarity with the several pieces of literature. In this manner, Kline obtains the relation between learning to make a new response to an old stimulus and the strength of the connection between the original response (correct author), and the given stimulus. Kline obtains negative transfer in every case, except when the initial learning is extremely imperfect.

Wylie teaches rats to make a negative response to light, sound, or pain, and then a negative response to a different one of the three stimuli; the rats must thus learn to make an old response to a new stimulus in an old environment. His results, arranged according to six different combinations, follow:

Light to sound, 244 percent positive transfer
Light to pain, 21 percent positive transfer
Sound to light, 33 percent positive transfer
Sound to pain, 54 percent positive transfer
Pain to light, 36 percent positive transfer
Pain to sound, 68 percent positive transfer

Bair, using a specially prepared typewriter, tests the ability of subjects to respond to two series of 55 colors (6 different colors) under two arrangements of a color keyboard. The three situations indicated below are provided. In passing from task 1 to 2, the subject makes an old type of response to a new list of stimuli; in passing from task 2 to 3, the subject makes a new type of response to an old list of stimuli; the former results in positive and the latter in negative transfer, thus lending support to the findings of Hunter, Pearce, Kline, and Wylie cited above. The results are given in repetitions required per task, at a constant rate of 104 strokes per minute.

1. Series *A*, arrangement *A*, 40 repetitions
2. Series *B*, arrangement *A*, 13 repetitions
3. Series *B*, arrangement *B*, 23 repetitions

III

In the third class of studies, *the stimulus-response terms in the experimental situation are differentiated, and the influence of two or more variables is examined.*

The present study belongs to this class. It is in fact the first and only study of its type which has as yet appeared. Previous authors have differentiated the stimulus-response terms. Previous authors have varied the degree of integration of the initial learning. But they have not examined the relation of similarity of certain ones of the $S_1R_1S_2R_2$ terms to learning transfer. Moreover, in no previous study has the combined influence of several kinds of variation been examined. We are therefore attempting to investigate in this study the relation and the interrelation of the following three variables to learning transfer:

Identity of certain ones of the $S_1R_1S_2R_2$ terms
Degree of integration of the initial learning
Similarity of certain ones of the $S_1R_1S_2R_2$ terms.

Technique

(1) *Learning Terms Defined*

Learning transfer relates to the effect of initial upon subsequent learning. In this investigation, the initial and subsequent learning are further differentiated into stimulus and response terms, which are designated initial stimulus, initial response, subsequent stimulus, subsequent response, $S_1R_1S_2R_2$.

(2) *General Nature of the Learning Material*

The learning material in these studies consists of nonsense syllables. The syllables may be similar, different, or identical; by arbitrary definition, 'similar' syllables are syllables which vary only in respect to the final letter, such as var, vam.

(3) *Description of the Conditions, Relative to*

(*a*) *Identities between Certain of the* $S_1R_1S_2R_2$ *Terms*

The relation of identity of the $S_1R_1S_2R_2$ terms is varied. In condition IX, the terms are all different. In conditions I, II, III, IV, the S_1 and S_2 are identical, which means that the subjects are required to learn a new response to an old

stimulus. In conditions V, VI, VII, VIII, the R_1 and R_2 are identical, which means that the subjects are required to learn to make an old response to a new stimulus.

(*b*) *Similarities between Certain of the* $S_1R_1S_2R_2$ *Terms*

In conditions I, V, IX, no similarities are present. In conditions II and VI, the initial stimulus and initial response are similar. In conditions III and VII, the subsequent stimulus and subsequent response are similar. In condition IV, the initial response and subsequent response are similar, and in condition VIII, the initial stimulus and the subsequent stimulus are similar.

(*c*) *Integration of the Initial Learning*

In each of the nine conditions, an initial list of syllables is presented to the subject either 0, 2, 6 or 12 times, regardless of the number of repetitions required to learn the initial list. By means of this variation, situations are provided in which the original material may be entirely unlearned, slightly learned, almost learned, completely learned, or overlearned. A subsequent list is then repeatedly presented until it is learned. It is assumed that a list is learned, when the subject can correctly anticipate the second member of each pair before it actually appears. The last repetition therefore is not counted. The following tables is a partial summary of the conditions.

SUMMARY OF CONDITIONS ILLUSTRATION OF IDENTITIES AND SIMILARITIES BETWEEN TERMS, IN THE DIFFERENT CONDITIONS

Condition	Initial learning		Subsequent learning		Relation of initial to subsequent material
	Stimulus S_1	Response R_1	Stimulus S_2	Response R_2	
I	req	kiv	req	zam	S_1S_2 identical
II	bij	bic	bij	tab	S_1S_2 identical, S_1R_1 similar
III	mir	ped	mir	miy	S_1S_2 identical, S_2R_2 similar
IV	tec	zox	tec	zop	S_1S_2 identical, R_1R_2 similar
V	lan	qip	fis	qip	R_1R_2 identical
VI	soj	soy	nel	soy	R_1R_2 identical, S_1R_1 similar
VII	zaf	qer	qec	qer	R_1R_2 identical, S_2R_2 similar
VIII	bes	yor	bef	yor	R_1R_2 identical, S_1S_2 similar
IX	xal	pom	cam	lup	all terms different

(4) *Rate of Presentation*

A 2-second interval is given per successive exposure.

(5) *Method of Presentation*

In all of the conditions, a variation of the paired-associates method is used, in which the subject is presented first with a given syllable, then with the syllable and its associate, and so forth. An illustrative list of syllables follows:

Initial S_1R_1 list (S_1 with R_1)		Subsequent S_2R_2 list (S_2 with R_2)	
req		fiz	
req	kiy	fiz	sej
taw		mip	
taw	rif	mip	boc
qix		bul	
qix	lep	bul	puw
wam		nic	
wam	bos	nic	git
zed		caj	
zed	dib	caj	lim

(6) *Length of the Lists*

It was necessary, in arranging the length of the lists for the various conditions, to make certain that they were long enough, in all cases, to require several repetitions in the learning. Conditions III and VII permitted more rapid learning than the other conditions, and hence longer lists were employed in these two conditions. Therefore, the length of the lists, as determined by pre-experimentation is 5 syllables in conditions I, II, IV, V, VI, VIII, IX, and 7 syllables in conditions III and VII.

(7) *Different Groupings of the Pairs of Syllables*

The members of each pair of syllables remain invariably together, but the order of the pairs in the list is varied in an irregular manner from trial to trial.. This arrangement, which provides for four different groupings of the pairs per list, follows:

Repetition of list	Sequence of presentation of the syllables per list				
1st	1	2	3	4	5
2d	4	1	5	3	2
3d	5	4	2	1	3
4th	2	5	1	3	4
5th, etc	1	2	3	4	5

(8) *Practice Effects Equalized*

The conditions of integration are regularly shifted per subject per cycle in a manner calculated to equalize the benefits to be derived from practice. The sequence is so adjusted that every subject studies each of the four conditions of integration under each of the four successive learning periods of the cycle.

(9) *Differences in Difficulty of Lists Adjusted*

The syllable lists are given in a sequence per subject per condition calculated to equalize their difficulty in learning. The sequence is so adjusted that each syllable list is learned 2 or more times under each of the conditions of integration, and under each of the four successive learning periods of a cycle group.

(10) *The Subjects*

Eighty-one college students served as subjects; nine are used in each condition, and each one is tested individually. Each subject is tested under each of the four conditions of integration, and this is repeated for four cycles. Thus sixteen periods of experimentation are required of each person, and as far as possible, these periods of experimentation are given one at a time, at the rate of one per day.

(11) *Apparatus*

The apparatus used in this study consists of a metronome, some adding machine paper, and a 12x12 inch cardboard in the center of which are two parallel 4½ inch slits ⅝ inches apart.

The metronome is used to provide 2-second intervals.

The adding machine paper is used as a convenient medium for presenting typewritten lists of syllables. In order to present four different groupings of lists of pairs, and in order to provide at least double spacing between successive items, rolls of approximately 36 inches in length are used.

The 12 x 12 inch cardboard is used in lieu of a memory drum. The syllable roll, concealed beneath the cardboard except for the small portion exposed between the two parallel

slits, is pulled forward by the experimenter at regular intervals of 2 seconds per exposure item.

Results

I

Relation of the following types of learning to transfer:

A. Learning to make a new response to an old stimulus.

B. Learning to make an old response to a new stimulus.

C. Learning to make a new response to a new stimulus.

TABLE I[2]

Relation to Transfer, of Learning to Make a New Response to an Old Stimulus, an Old Response to a New Stimulus, a New Response to a New Stimulus

Condition	Mean repetitions required in the subsequent learning under conditions of 0 and 12 repetitions of initial learning		Mean values letting the condition 0 value = 100	
	0	12	0	12
I	10.3	11.2	100	109
V	14.4	9.0	100	63
IX	9.9	8.3	100	84

[2] Each mean value in this and the following tables is an average of 36 measures.

The probable errors are omitted from the tables, chiefly for reasons of convenience. The P.E. of the difference of given means is stated in the discussion, in a few crucial instances; the subsequent findings hinge upon these values.

The great differences between the 0 integration values in this and the following tables, are not of any particular significance inasmuch as

a. different persons serve as subjects in each of the nine conditions of the study,

b. different lists of memory material are used in each of the nine conditions of the study,

c. longer lists of memory material are used under some of the conditions than under others,

d. learning is easier under some of the conditions than under others, since the pairs of syllables are similar in some conditions and different in others.

The data of Table I indicate that learning to make a new response to an old stimulus is not benefited by the initial learning of 12 repetitions; in fact the number of repetitions required increases from 10.3 to 11.2 indicating a negative transfer of 100 : 109, although the difference of 0.9 repetitions is statistically insignificant.

Learning to make an old response to a new stimulus is

materially benefited by the initial learning; the number of repetitions required decreases from 14.4 to 9.0 indicating a positive transfer of 100 : 63. The difference of 5.4 repetitions is statistically significant since it is more than three times the P.E. of the difference, 1.5.

Learning to make a new response to a new stimulus is slightly benefited by the initial learning; the number of repetitions required decreases from 9.9 to 8.3 indicating a positive transfer of 100 : 84. The difference of 1.6 repetitions is too small to be of much significance since the P.E. of the difference is 1.5.

In general the data indicate that there is a marked positive transfer in learning to make an old response to a new stimulus (100 : 63), a slight positive transfer in learning to make a new response to a new stimulus (100 : 84), and a slight negative transfer in learning to make a new response to an old stimulus (100 : 109).

II

Relation of the degree of integration of the initial learning to transfer in

A. Learning to make a new response to an old stimulus.

B. Learning to make an old response to a new stimulus.

TABLE II

INFLUENCE OF THE DEGREE OF INTEGRATION (0, 2, 6, 12 REPETITIONS) OF THE INITIAL LEARNING UPON THE SUBSEQUENT LEARNING. RELATIVE TO LEARNING TO MAKE A NEW RESPONSE TO AN OLD STIMULUS, AN OLD RESPONSE TO A NEW STIMULUS, A NEW RESPONSE TO A NEW STIMULUS.

Condition	Mean repetitions required in the subsequent learning under conditions of 0, 2, 6, 12 repetitions of initial learning				Mean values letting the condition 0 value = 100			
	0	2	6	12	0	2	6	12
I	10.3	12.1	11.9	11.2	100	117	116	109
V	14.4	16.6	12.0	9.0	100	115	83	63
IX	9.9	9.9	10.7	8.3	100	100	108	84

In learning to make a new response to an old stimulus, the number of repetitions required varies with the degree of integration of the initial learning: 2 repetitions causes a negative transfer of 100 : 117; 6 repetitions causes a negative

transfer of 100 : 116; and 12 repetitions causes a negative transfer of 100 : 109.

In learning to make an old response to a new stimulus, 2 repetitions of the initial learning causes a negative transfer of 100 : 115; 6 repetitions causes a positive transfer of 100 : 83; and 12 repetitions causes a positive transfer of 100 : 63.

In learning to make a new response to a new stimulus, 2 repetitions of the initial learning causes a zero transfer; 6 repetitions causes a negative transfer of 100 : 108; and 12 repetitions causes a positive transfer of 100 : 84.

The difficulty of the learning tends to decrease, as the amount of the initial learning is increased from 2 to 6 to 12 repetitions (compare 117 with 116 with 109; also 115 with 83 with 63; also 100 with 108 with 84); this is true moreover, regardless of whether or not similarities are present (see tables III and IV, and compare also 101,90,90; 127,123,102; 102,101,80; 103,81,77; 66,56,40; 84,64,44); the decreased difficulty may show itself in terms of a decreasing negative transfer (117,116,109), or in terms of a shift from negative to positive transfer (115,83,63), or in terms of an increasing positive transfer, if similarities are involved (see table IV, 84,64,44).

III

Relation of the similarity between the $S_1R_1S_2R_2$ terms of the initial and subsequent material, to transfer in

A. Learning to make a new response to an old stimulus.

B. Learning to make an old response to a new stimulus.

TABLE III

INFLUENCE OF SIMILARITY OF TERMS INVOLVED IN INITIAL AND SUBSEQUENT MATERIAL UPON THE SUBSEQUENT LEARNING WHEN LEARNING TO MAKE A NEW RESPONSE TO AN OLD STIMULUS

Condition	Mean repetitions required in the subsequent learning under conditions of 0, 2, 6, 12 repetitions of initial learning				Mean values letting the condition 0 value = 100			
	0	2	6	12	0	2	6	12
I	10.3	12.1	11.9	11.2	100	117	116	109
II	11.1	11.2	10.0	10.0	100	101	90	90
III	8.2	10.4	10.1	8.4	100	127	123	102
IV	9.6	9.8	9.7	7.7	100	102	101	80

Learning to make a new response to an old stimulus when no similarities are present, is slightly hindered by 12 repetitions of initial learning, 100 : 109. If the stimulus and response terms of the subsequent learning are similar, transfer is practically zero, 100 : 102. If the stimulus and response terms of the initial learning are similar, a slight positive transfer occurs, 100 : 90. If the response terms of the initial and subsequent learning are similar, a further slight increment of positive transfer is added, 100 : 80. In general, the learning is least facilitated when no similarities are present (compare 109 with 90,102,80), and most facilitated when the initial and subsequent responses are related (compare 80 with 109,90,-102); the first gives a slight degree of negative transfer, the latter a moderate degree of positive transfer. The relative effectiveness of the four conditions of experimentation varies irregularly with the degree of integration of the initial learning. Under the best conditions, a slight positive transfer occurs; under the poorest conditions, a slight negative transfer occurs.

TABLE IV

INFLUENCE OF SIMILARITY OF TERMS INVOLVED IN INITIAL AND SUBSEQUENT MATERIAL UPON THE SUBSEQUENT LEARNING, WHEN LEARNING TO MAKE AN OLD RESPONSE TO A NEW STIMULUS

Condition	Mean repetitions required in the subsequent learning under conditions of 0, 2, 6, 12 repetitions of initial learning				Mean values letting the condition 0 value = 100			
	0	2	6	12	0	2	6	12
V	14.4	16.6	12.0	9.0	100	115	83	63
VI	12.8	13.2	10.4	9.8	100	103	81	77
VII	13.1	8.7	7.3	5.3	100	66	56	40
VIII	13.5	11.4	8.7	5.9	100	84	64	44

Learning to make an old response to a new stimulus when no similarities are present, is materially aided by an initial learning of 12 repetitions; the repetitions required decrease from 14.4 to 9.0, a positive transfer of 100 : 63. If the stimulus and response terms of the initial learning are similar, the amount of positive transfer slightly decreases, 100 : 77. If the stimulus and response terms of the subsequent learning are similar, the amount of positive transfer is greatly in-

creased, 100 : 40; so also, if the initial stimulus and the subsequent stimulus are similar, 100 : 44. In general, the positive transfer is greatest when the subsequent stimulus is similar either to the subsequent response, 100 : 40 or to the initial stimulus, 100 : 44. The positive transfer is least when the similarities are restricted to the terms of the initial learning, 100 : 77; in this latter case, the similarity is even a disadvantage; compare 100 : 77 with 100 : 63.

IV

Summary of results, concerning the conditions of transfer of training:

(1) Learning to make an old response to a new stimulus almost invariably gives marked positive transfer, whereas learning to make a new response to an old stimulus usually results in a slight degree of negative transfer; this is true regardless of whether the

Integration is controlled and the similarity controlled
Integration is varied and the similarity controlled
Integration is controlled and the similarity varied[3]
Integration is varied and the similarity varied.[3]

(2) The difficulty of subsequent learning tends to decrease as the amount of the initial learning is increased from 2 to 6 to 12 repetitions, regardless of whether learning to make a new response to an old stimulus, or learning to make an old response to a new stimulus; this decrease may be in terms of a decreasing negative transfer, in terms of a shift from negative to positive transfer, or in terms of an increasing positive transfer.

(3) The effects of similarity vary as follows:

(*a*) When learning to make a new response to an old stimulus, the learning is least facilitated when no similarities are present, and most facilitated when the initial and subsequent response are similar; when least facilitated, a slight degree of negative transfer occurs, and when most facilitated, a moderate degree of positive transfer occurs.

[3] Compare conditions I and V, II and VI, III and VII, IV and VIII respectively, of tables III and IV.

(*b*) When learning to make an old response to a new stimulus, the learning is most facilitated when the subsequent stimulus is similar either to the subsequent response or to the initial stimulus, and least facilitated when the similarities are restricted to the factors of the initial learning; in this latter case, the similarity is even a disadvantage. But in all cases, marked positive transfer occurs.

Interpretation

I

Our data (condition I) support the findings of both Hunter and Pearce; we find that learning to make a new response to an old stimulus is accompanied by a small degree of negative transfer, when no similarities are involved. Hunter and Pearce find a very marked negative transfer in training rats to turn to the right in a T-maze when given a light, to the left when not given the light, and then to the left when given the light and to the right when not given the light. The chief difference between the experiments, is that in the Hunter and Pearce studies the new and old responses are antagonistic muscular reactions, *i.e.*, the rats are taught first to turn to the right when given the light, and then later to turn to the left when given the light, etc., whereas in our condition I, the new and old responses (syllables) are different but not necessarily antagonistic reactions. This difference undoubtedly accounts in part for the greater negative transfer in the rat studies.

Our data (condition I) support in part the findings of Kline; in our study, learning to make a new response to an old stimulus invariably results in negative transfer, regardless of the degree of integration of the initial learning; the same is true of Kline's results, except when the integration of the initial learning is extremely imperfect (1 percent to 10 percent). In both studies, the greatest degree of integration of the initial learning, causes a negative transfer in the subsequent learning, but the amount of this negative transfer is less than that which results when the integration of the initial learning is somewhat less complete. Our data fail to support

the findings of Kline in one respect: when the degree of the initial learning is meager, we usually obtain negative transfer, whereas Kline obtains a slight degree of positive transfer; this is in fact the only condition under which Kline obtains positive transfer.

Our data (condition V) support the findings of Wylie. In both studies, learning to make an old response to a new stimulus is accompanied by a marked degree of positive transfer, when the integration of the initial learning is complete. Wylie taught rats to make a negative response, first to either light, sound, or pain, and then to a different one of the three stimuli.

Our data support the findings of Bair. In both studies, learning to make an old response to a new stimulus gives positive transfer, when the integration of the initial learning is complete (condition V), whereas learning to make a new response to an old stimulus gives negative transfer (condition I).

Our data also support the findings of Ho, who studies the effect of varying degrees of integration of maze learning upon the learning of a second similar maze; he finds that an increasing degree of integration of the initial learning is accompanied by an increasing positive transfer. Wiltbank, in an earlier study of the same problem, when using the criterion of average time per trial, obtains about 75 percent positive transfer regardless of the degree of integration of the first maze learning; but when measured in average number of trials, or in average errors per trial, positive transfer is obtained only after 16 or more trials upon the first maze.

II

Thorndike's theory of transfer is one of the more ambitious attempts to explain transfer. Thorndike says:[4]

"The problem, which is clearly one of psychological fact, may be best stated in psychological terms as follows: How far does the training of any mental function improve other mental functions? . . . The answer which I shall try to defend is that a change in one function alters another only insofar as the two functions have as factors identical elements. The change in the second function is in amount that due to the

[4] Thorndike, E. L., Educational Psychology, Briefer Course, 268–269.

change in the elements common to it and the first. . . . By identical elements are meant mental processes which have the same cell action in the brain as their physical correlate. It is of course often not possible to tell just what features of two mental abilities are thus identical."

The Thorndike theory was put forward primarily to explain positive transfer, inasmuch as this kind of transfer was practically the only kind to be explained, at the time of the formulation of the theory. The theory attempts to explain experimental data such as the following:

Swift in a cross education study employs ball-tossing and obtains positive transfer.

Scripture, Smith, and Brown in a cross education study, measure the ability of subjects to insert a needle into a small aperture; a positive transfer of 25 percent is obtained.

Starch in a study of cross education, employs mirror tracing, and obtains a 90 percent positive transfer.

Winch finds a positive transfer in the memory ability of girls.

Fracker in a study of memory of the order of four tones, obtains positive transfer.

Thorndike and Woodworth testing the ability of subjects to estimate the areas of different shapes, before and after training in judging the area of rectangles, find positive transfer in most of the cases.

Thorndike and Woodworth find that training in marking out words with given letters, aids 10 percent to 35 percent in another list in which different letters are used.

Starch in a study of transfer in arithmetic, tests individuals in simple mathematical procedures, before and after training them in mental multiplication. He finds a positive transfer of 20 percent to 40 percent.

Judd's experiment with boys who throw at a target under water, gives positive transfer. The boys to whom refraction is explained, progress more rapidly than the uninstructed boys.

Webb, testing both rats and human beings, seeks to ascertain whether there is transfer in passing from the learning of one maze to that of another. Webb's results show the

presence of positive transfer to a marked degree in all but one or two instances.

Wiltbank in a study of transfer, runs rats through a series of mazes. In general, his results give positive transfer from maze to maze.

Thorndike's theory also attempts to account for instances of zero transfer, for it implies that where there are no identical elements between the two functions, the change in the second function will be nil. The experiments of James, Bagley, Thorndike and Woodworth deserve mention at this point.

James, testing himself in poetry-memorizing, before and after memorizing passages from another type of poetry, finds that memory training does not aid; in fact James memorized passages a bit faster before rather than after the memory training.

Bagley teaches pupils to produce neat arithmetic papers, and finds that neatness fails to carry over to other subjects; the spelling and language papers fail to show even slight improvement in neatness.

Thorndike and Woodworth train subjects in estimating the length of lines of ½ to 1½ inches; although the subjects improve about 25 percent during the training, they fail to show any improvement in the estimation of lines of 6 to 12 inches as a result of the training.

The Thorndike theory is silent concerning instances of negative transfer such as are found in the experiments of Hunter, Pearce, and others.

Both Hunter and Pearce, using a T-maze, train rats first to turn to the right when given a light, to the left when not given the light, and then to the left when given the light, to the right when not given the light. Their results show conclusively that learning the first task greatly hinders the learning of the second task.

The silence of the Thorndike theory concerning negative transfer, appears to have given rise to the Poffenberger extension of the Thorndike theory. This 'Thorndike-Poffenberger' theory may be summed up in three general propositions:

I. Where there are no identical bonds between stimulus

and response, the influence will be neither positive nor negative.

II. Where there are identical elements, there will be positive transfer.

III. Where bonds are broken in the formation of new groupings, there will be negative transfer.[5]

Poffenberger, studying the influence of improvement in one simple mental process upon other related processes, seeks to determine the influence of training in cases which vary from close similarity between the two successive tasks, to instances in which there is marked antagonism between the two successive tasks. His conclusions are stated in the above three propositions. Poffenberger trains four subjects during the course of nine days, in each of these four tests: color naming, opposites, number checking, and addition; before and after this training, the subjects together with a control group are tested in each of the following seven tests: 1 form naming test, 1 adjective-noun test, 2 number group checking tests, and 3 calculation tests.

If we assume with Poffenberger that "where bonds are broken in the formation of new groupings, there will be negative transfer," then evidently no bonds are broken in Wylie's study in which rats are taught to make an old response to a new stimulus, since he obtains positive transfer. The same may be said of our conditions VI, VII, VIII.

But evidently bonds are broken in Hunter's study in which rats are taught to make a new response to an old stimulus, since he obtains negative transfer. The same may be said of our condition I.

The Thorndike-Poffenberger theory is weak and unstable, since it offers little more than the dogmatic implication that bonds are broken if negative transfer is obtained, and not broken if positive transfer is obtained. The theory is not subject to experimental test, if we are to assume that negative transfer is a cue to broken bonds, and vice versa.

Moreover, no a priori assumption is justified that bonds are or are not broken under any conditions of transfer. In

[5] Poffenberger, A. T., Influence of Improvement in One Simple Mental Process upon other Related Processes, *J. Educ. Psychol.*, 1915, 6, 474.

fact there is no proof that bonds are not broken in learning to make an old response to a new stimulus, under which conditions, positive transfer usually occurs.

The Thorndike-Poffenberger theory represents little more than a guess concerning the 'neurology' of transfer. It gives no basis for prediction in a given case whether positive or negative transfer will occur. But in our study, the results are formulated in a set of laws which have predictive value, since they state some of the main objective conditions under which positive and negative transfer may be expected to occur. The following section constitutes our set of laws.

III

Laws Concerning the Conditions of Transfer of Training

(1) Learning to make an old response to a new stimulus results in a marked degree of positive transfer.

(2) Learning to make a new response to a new stimulus results in a slight degree of positive transfer.

(3) Learning to make a new response to an old stimulus results in a slight degree of negative transfer.

(4) Introducing similarities between two or more of the $S_1R_1S_2R_2$ terms increases positive transfer, and decreases negative transfer.

(5) With increasing degrees of integration of the initial learning, there is an increase in the amount of positive transfer, and a decrease in the amount of negative transfer; where the amount of negative transfer is slight, it shifts to positive transfer.

References

1. Bagley, W. C., *Educative Process*. New York: Macmillan, 1924, 208.
2. Bair, J. H., The practice curve, *Psychol. Rev. Monog.*, 1913, **5**, No. 19, 14–25.
3. Coover, J. E., and Angell, F., General practice effect of special exercise, *Amer. J. Psychol.*, 1917, **18**, 328–340.
4. Fracker, G. C., On the transference of training in memory, *Psychol. Rev. Monog.*, 1908, **9**, No. 38, 56–102.
5. Ho, Y. H., Transfer and the degree of integration. Doctor's Thesis on File in the Library of the University of Chicago (1927).
6. Hunter, W. S., Habit interference in the white rat and in human subjects, *J. Comp. Psychol.*, 1922, **2**, 29–59.
7. James, W., *Principles of Psychology*. New York: Henry Holt, 1890, **1**, 666–667.
8. Judd, C. H., The relation of special training to general intelligence, *Educ. Rev.*, 1908, **36**, 28–43.

9. JUDD, C. H., *Psychology of High School Subjects.* New York: Ginn, 1915, 269–272.
10. KLINE, L. W., Associative inhibition, *J. Exper. Psychol.*, 1921, **4**, 270–299.
11. KLINE, L. W., AND OWENS, W. A., Preliminary report of a study in learning process involving feeling tone, transference, and interference, *Psychol. Rev.*, 1913, **20**, 206–245.
12. PEARCE, B. D., A note on the interference of visual habits in the white rat, *J. Animal Behav.*, 1917, **7**, 169–177.
13. POFFENBERGER, A. T., Influence of improvement in one simple mental process upon other related processes, *J. Educ. Psychol.*, 1915, **6**, 459–474.
14. RICHARDSON, F., A study of sensory control in the rat, *Psychol. Rev. Monog.*, 1909 1911, **12**, No. 1, 104.
15. SCRIPTURE, E., SMITH, T., AND BROWN, E., On the education of muscular control and power, *Yale Psychol. Studies*, 1894.
16. STARCH, D., A demonstration of the trial and error method of learning, *Psy. Bull.*, 1910, **7**, 20–23.
17. STARCH, D., Transfer of training in arithmetical operations, *J. Educ. Psychol.*, 1911, **2**, 306–311.
18. SWIFT, E. J., *Mind in the Making.* New York: Charles Scribner's Sons, 1908, 169–219.
19. THORNDIKE, E. L., *Educational Psychology Briefer Course.* New York: Teachers College, Columbia University, 1914, 268–269.
20. THORNDIKE, E. L., AND WOODWORTH, R. S., The influence of improvement in one mental function upon the efficiency of other mental functions, *Psychol. Rev.*, 1901, **8**, 247–261, 384–395, 553–564.
21. WATERS, R. H., Influence of tuition on ideational learning. Doctor's Thesis on File in the Library of the University of Chicago (1928), 18–20.
22. WEBB, L. W., Transfer of training and retroaction. *Psychol. Rev. Monog.*, 1917, **24**, No. 3, pp. 90.
23. WILTBANK, R. T., Transfer of training in white rats upon various series of mazes, *Behav. Monog.*, 1919, **4**, No. 1, pp. 65.
24. WINCH, W. H., The transfer of improvement in memory in school children, *Brit. J. Psychol.*, 1908, **2**, 284–294.
25. WYLIE, H. H., Transfer of response in the white rat, *Behav. Monog.*, 1919, **3**, No. 5, Pp. 66.

(Manuscript received February 25, 1932)

THE EFFECT OF THE INITIAL CHANCES FOR RIGHT RESPONSES UPON THE EFFICACY OF INTENSIFIED REWARD AND OF INTENSIFIED PUNISHMENT[1]

BY IRVING LORGE

Teachers College, Columbia University

With the Assistance of the Staff of the Division of Psychology of the Institute of Educational Research

The potency of reward has been demonstrated by experiments in learning to associate a stimulus with one of several responses ['32 Thorndike]. The task used by Thorndike was, in one instance, the associating of a Spanish word with an English word meaning the same as the Spanish word; and, in the other, the connecting of a visual nonsense stimulus with the correct one of ten actions. Only one of the given five English meanings was correct for any Spanish word, and only one of the possible ten actions was correct for any visual stimulus. It was felt that failure was overweighted in allowing but one correct response and several wrong responses. In experiments similar to those of Thorndike with intensified punishment and with intensified reward ['33 Lorge] the multiple choice experiment was changed so that the initial chance of a right, or of a wrong response was equal. "Equalization of chance at initial trial to avoid the consequence of overweighting failure and success was secured by reducing the number of choices from five to four, and by arbitrarily having two rights instead of one. In any line of nonsense words to meaningful English words, a subject had an equal chance of getting a right or a wrong response in trial one."

The results of these experiments were similar to those obtained by Thorndike. A punished response does not eliminate itself faster than chance. The total improvement is assignable to the potency of reward.

[1] This investigation is made possible by a grant from The Carnegie Corporation.

Experiments have been designed in which the initial chance of a right response is not equal to that for a wrong response. The same materials are used with the same technic of rewarding the choice of a right response with an announcement of *Right* and a slight bonus, and of punishing the choice of a wrong response with an announcement of *Wrong* and an electric shock. The difference between the new experiments which are to be reported in this paper and the experiments which have been reported is in the initial chance of obtaining a right response.

In Experiment 5, the chances of obtaining a right response are three out of four; in Experiment 6, the chance of a right response is one out of four. It is possible to compare the effectiveness of reward and punishment under conditions in which the initial chances of obtaining a right response are three out of four, two out of four,[2] and one out of four.

Experiments Five and Six

A nonsense code word followed by four English words on the same line constituted the stimulus unit. Twenty such stimulus units were presented on a page. Presentation of five identical pages was called a set. Two such sets were presented to the group participating in Experiment 5, and two such sets were presented to the group of Experiment 6.

Each subject was seated comfortably at a table. Dry electrodes of brass screening leading from an inductorium were placed on the subject's non-writing hand—one over the three middle fingers, the other about the palm.[3] The directions for Experiment 6 differed slightly from those of Experiment 5. In Experiment 5, there were three correct equivalents for the code words and one word which was wrong. In Experiment 6, there was but one correct equivalent for the code words and three words which were wrong. For Experiment 5, the directions were:

"Look at the first word in the first line. It is a code word which has three equivalents. The three equivalents are among the group of four words following the code word. When we begin, you will choose one word that you think is an equivalent for the code word at the beginning of the line. If you are right, I will say *Right* and give you one of these little tokens. Each is worth one-tenth of a cent. If you are wrong, I shall

---

[2] The data for this comparison are obtained from the results of Experiment 2 ['33 Lorge].

[3] In this experiment, as in the experiments reported on the efficacy of and intensified reward and of intensified punishment, the announcement of wrong was intensified by giving an electric shock through an inductorium operating on four and a half volts, which was strong enough to have caused the person to lift his hand ten times in succession. The determination of shock was made, of course, for each subject prior to the actual experimentation. The announcement of right was intensified by giving a person a token worth one-tenth of a cent for every response that was correct.

say *Wrong* and give you a slight electric shock. The shock will not hurt you, but will be just uncomfortable enough to make you remember that the response was wrong. As soon as I say *Right* or *Wrong*, you will immediately choose an equivalent for the second code word on the next line and draw a line under it, and so on. At the end of the period, I will give you one cent for every ten tokens you have earned. The task is to see how quickly you can learn one equivalent for each code word, so as to learn many code equivalents and so make more money, and receive fewer shocks.

For Experiment 6, the only difference in the directions was in the opening sentences: "Look at the first word in the first line. It is a code word which has but one equivalent. The one equivalent is among the group of four words following the code word" (after which the directions were the same as those for Experiment 5).

Ten subjects, graduate students at Teachers College, participated in Experiment 5 during April and May, 1931. All subjects completed the five trials of both sets *A* and *B* at one sitting.

Ten subjects, graduate students at Teachers College, participated in Experiment 6 during May, 1931. All subjects completed the five trials of both sets at one sitting.

The analyses of the data were the same as those utilized in the study of the efficacy of intensified reward and punishment ['33 Lorge].

The influence of the statement *Wrong* and the concomitant electric shock was measured by the excess over chance of instances in which a response differed at the second trial from the initially punished response.

Let X_s be a particular wrong response punished at any trial,

Let A be any response differing from the preceding response,

Let the order in which the symbols [4] are written represent the history of the responses to a particular stimulus, and,

Let G_x be the *a priori* chance that a response other than the preceding response will occur.

Then, the measure of the influence of a single punishment will be the relative magnitude of

$$\frac{X_sA}{X_sX_s + X_sA}.$$

[4] Thus X_sX_sA means that in the first trial, the response was X_s; in the second trial, the response was again X_s; but in trial 3 the response was A, a response differing from X_s. X_sX_s may be X_1X_1 or X_2X_2.

If at the first trial a subject responded to X_s, he can make at the second trial, in addition to X_s, three other responses. Thus the chances of obtaining a response other than the original wrong response are three out of four. G_x is, therefore, .75. This allowance, G_x, then is applied to correct for the influence of chance.

By extension, the influence of two successive punishments for the same wrong will be measured by the relative magnitude of

$$\frac{X_sX_sA}{X_sX_sX_s + X_sX_sA} - G_x$$

and for three successive punishments, and for four successive punishments for the same wrong, be measured by the relative magnitude of

$$\frac{X_sX_sX_sA}{X_sX_sX_sX_s + X_sX_sX_sA} - G_x$$

and

$$\frac{X_sX_sX_sX_sA}{X_sX_sX_sX_sX_s + X_sX_sX_sX_sA} - G_x,$$

respectively.

In a similar manner, a measure of the influence of a single reward can be obtained. If chance alone were operating to determine the response in the second trial, a C_s used in trial 1 for a particular stimulus would be the response in one-fourth of the cases. Any excess over this proportion is due to, and is a measure of, the influence of a single rewarded response.

Let C_s be a specific right rewarded at any trial, and

A be any response differing from the preceding response,

Let G_c represent the *a priori* chance that the same response will be repeated, and

Let the order in which the symbols are written represent the history of the responses to a particular stimulus.

The formulæ for the measurement of the influence of successive rewards are:

Rewards for the number of successive repetitions and same right response	Formulæ
one	$\frac{C_sC_s}{C_sC_s + C_sA} - G_c$
two	$\frac{C_sC_sC_s}{C_sC_sC_s + C_sC_sA} - G_c$
three	$\frac{C_sC_sC_sC_s}{C_sC_sC_sC_s + C_sC_sC_sA} - G_c$
four	$\frac{C_sC_sC_sC_sC_s}{C_sC_sC_sC_sC_s + C_sC_sC_sC_sA} - G_c$

These analyses result in tables such as Table 22 and 23.

The group of ten subjects learning an associate for the nonsense word when one response was right and three were wrong (hereafter called 3W 1R) had an average of 4.6 right responses in trial one, and 7.2 right responses in trial 5 for set *A* indicative of selective learning. This same group on set *B* had an average of 4.9 and 7.1 right responses in trials 1 and 5 respectively.

The other group learning an associate for the nonsense words when three responses were right and one was wrong (hereafter called 3R 1W) had on set *A* an average of 15.1 and 18.8 right response in trials 1 and 5 respectively, and on set *B* an average of 15.0 and 18.7 right responses.

The criterion of selective learning having been met the data is utilized for a reward-punishment comparison.

Tables 22 and 23 are clear in showing that a C_s which was used as a response to a stimulus code word in trial one is more likely to be used as the response in trial 2, than is likely by chance. The evidence of these tables is just as clear in indicating that an X_s used as response in trial one is none the less likely to be used in trial 2. The same X_s occurs more often in trial 2 than chance expectation.

The initial probability of right response effects the potency of the reward. The higher the initial probability of choosing a right response, the greater the value of a single reward. When the initial chances for right are three out of four, the value of a single reward is +45 as against +29 when the chances are one out of four. The potency of a single punish-

TABLE 22

EXPERIMENT 5. GROUP 3R 1W

Code words—Sets *A* and *B*	The summary of the frequencies of C_sC_s, C_sA, X_sX_s, X_sA, etc. for trials 1, 2, 3, 4, and 5 with subtractions for *a priori* chance			
	No. of responses	Percent all	The excess in percent over chance at the trial for repetition of the same right response	The excess in percent over chance at the trial for a response other than the particular wrong response
At trial 2				
C_sC_s	211	70	+45	
C_sA	90	30		
X_sX_s	35	35		
X_sA	64	65		−10
At trial 3				
$C_sC_sC_s$	178	84	+59	
C_sC_sA	33	16		
$X_sX_sX_s$	17	48		
X_sX_sA	18	52		−23
At trial 4				
$C_sC_sC_sC_s$	171	96	+71	
$C_sC_sC_sA$	7	04		
$X_sX_sX_sX_s$	9	53		
$X_sX_sX_sA$	8	47		−28
At trial 5				
$C_sC_sC_sC_sC_s$	167	98	+73	
$C_sC_sC_sC_sA$	4	02		
$X_sX_sX_sX_sX_s$	5	55		
$X_sX_sX_sX_sA$	4	45		−30

ment is −10 for the 3R 1W group and +04 for the 1R 3W group. The value of one, two, three and four successive rights for the same right response is given in Table 24. The value of the reward is higher in every instance for the 3R 1W group. The value of the reward for the 2R 2W group is higher than in the 1R 3W group. The weighted average value of a single right allows a better contrast. The values are +26, +23, and +21 when the initial probability for a right is three, two, and one out of four, respectively.

The potency of a punishment is less, the higher the initial probability of obtaining a right response. The data of Table

TABLE 23

EXPERIMENT 6. GROUP 1R 3W

Code words—Sets *A* and *B*	The summary of the frequencies of C_sC_s, C_sA, X_sX_s, X_sA, etc. for trials 1, 2, 3, 4, and 5 with subtractions for *a priori* chance			
	No. of responses	Percentall	The excess in percent over chance at the trial for repetition of the same right response	The excess in percent over chance at the trial for a response other than the particular wrong response
At trial 2				
C_sC_s	51	54	+29	
C_sA	44	46		
X_sX_s	64	21		
X_sA	241	79		+04
At trial 3				
$C_sC_sC_s$	36	71	+46	
C_sC_sA	15	29		
$X_sX_sX_s$	21	33		
X_sX_sA	43	67		−08
At trial 4				
$C_sC_sC_sC_s$	28	78	+53	
$C_sC_sC_sA$	8	22		
$X_sX_sX_sX_s$	9	43		
$X_sX_sX_sA$	12	57		−18
At trial 5				
$C_sC_sC_sC_sC_s$	25	90	+65	
$C_sC_sC_sC_sA$	3	10		
$X_sX_sX_sX_sX_s$	3	33		
$X_sX_sX_sX_sA$	6	67		−08

24 and the weighted average value of a single wrong indicate that something in the general situation in which right responses are favored, affects the potency of punishments.

The situation in which three responses out of four are wrong may be better comprehended when it is realized that on the first page of a set the subject is told *Wrong* on the average, fifteen times. It may be that the effects of these many announcements of *Wrong* and their concomitant electric shocks are summated in the subject so as to cause responses to be changed no matter whether the responses from which they change are right or wrong.

TABLE 24

THE EFFICACY OF SUCCESSIVE RIGHTS AND OF SUCCESSIVE WRONGS WHEN THE CHANCES OF RIGHTS ARE 3 OUT OF 4, 2 OUT OF 4 AND 1 OUT OF 4 RESPECTIVELY

Number of successive rights	Group 3R 1W	Group 2R 2W	Group 1R 3W
One	+45	+29	+29
Two	+59	+52	+46
Three	+71	+64½	+53
Four	+73	+71½	+65
Number of successive wrongs			
One	−10	−11	+04
Two	−23	−16	−08
Three	−28	−23	−18
Four	−30	−41	−08

The data of Table 24 are in some instances based on very few cases. The average efficacy of a single right and of a single wrong based upon a weighted [5] average, follows:

Group	Weighted average of a single right	Weighted average of a single wrong
3R 1W	+26	−10
2R 2W	+23	−09
1R 3W	+21	+00½

The important point to note, however, is that despite the increased tendency to change responses, the efficacy of a single reward is considerably above chance, whereas that of a single punishment is close to chance and in the wrong direction. Even in the favored instance, the improvement is a function of the rewarded and not of the punished responses.

The general influence of a single right is lower in the 1R 3W than in either the 2R 2W or 3R 1W groups. The influence of a single wrong is practically zero in the 1R 3W group. In the

[5] The average weighted value of a single right is obtained by weighting the values in excess of change by the number of instances determining such value and summating for the four sets of such determinations that are obtained, and dividing through by the number of cases weighted by the number of rights. In a similar manner, the average weighted value of a single wrong may be obtained. The determination below indicates the manner in which the weighted average value of a single right is obtained from the Groups 3R 1W from the date of Table 22.

$$\frac{(211+90)\,(45)+(178+33)\,(59)+(171+7)\,(71)+(167+4)\,(73)}{1(211+90)+2(178+33)+3(171+7)+4(167+4)}=26.3.$$

other two groups, the value of a single wrong is demonstrably negative.

Although the probability is small that fore-knowledge may influence results, prejudice or system may have an effect. Ideas about the stimulus, or a preference among responses may operate to favor rewards, and to negate the influence of punishment.

Prejudice and system may be controlled by considering only those stimulus words for which the responses were right in trial 1 and for which the responses were wrong in trial 2, or those stimulus words the responses for which were wrong in trial 1, and for which the responses were right in trial 2.

We utilize the data for the stimulus code words for which the responses varied in trial 1 and 2. These data are freed from the operation of prejudice and system, for only those stimuli are considered in which the first response is not maintained through trial two.

The formulæ which apply as a measure of the influence of successive rewards and successive punishments are:

Successive effects for the same response	Measure of the influence of successive rights	Measure of the influence of successive wrongs
One	$\frac{X\,C_sC_s}{X\,C_sC_s + XC_sA} - G_c$	$\frac{C\,X_sA}{C\,X_sX_s + C\,X_sA} - G_x$
Two	$\frac{X\,C_sC_sC_s}{X\,C_sC_sC_s + X\,C_sC_sA} - G_c$	$\frac{C\,X_sX_sA}{C\,X_sX_sX_s + C\,X_sX_sA} - G_x$
Three	$\frac{X\,C_sC_sC_sC_s}{X\,C_sC_sC_sC_s + X\,C_sC_sC_sA} - G_c$	$\frac{C\,X_sX_sX_sA}{C\,X_sX_sX_sX_s + C\,X_sX_sX_sA} - G_x$

After removing the influence of prepotency and prejudice, the values of reward and punishment are derived (Tables 25 and 26). The data are evidence that right responses are retained in excess of chance expectation. Wrong responses, however, are not eliminated even up to chance expectation.

Table 27 is comparable with Table 24. The essential difference between the tables is that in Table 27 prepotency, prejudice and system have been freed from the data. The higher the initial chance of right, the higher the value of the reward. There is no ambiguity in this finding. The values

TABLE 25

EXPERIMENT 5. GROUP 3R 1W

Code words—Sets *A* and *B*	The summary of the frequencies of $X_aC_sC_s$, X_aC_sA, $C_aX_sX_s$, C_aX_sA, etc. for trials 1, 2, 3, 4, and 5 with subtractions for *a priori* chance			
	No. of responses	Percent all	The excess in percent over chance at the trial for repetition of the same right response	The excess in percent over chance at the trial for a response other than the particular wrong response
At trial 3				
$X_aC_sC_s$	50	80	+55	
X_aC_sA	13	20		
$C_aX_sX_s$	6	29		
C_aX_sA	15	71		−04
At trial 4				
$X_aC_sC_sC_s$	43	86	+61	
$X_aC_sC_sA$	7	14		
$C_aX_sX_sX_s$	3	50		
$C_aX_sX_sA$	3	50		−25
At trial 5				
$X_aC_sC_sC_sC_s$	43	100	+75++	
$X_aC_sC_sC_sA$	0	00		
$C_aX_sX_sX_sX_s$	2	67		
$C_aX_sX_sX_sA$	1	33		−42

of a right for the 3R 1W group are consistently higher than those for the 2R 2W group. The values of the 2R 2W group are consistently higher than the values of rights for the 1R 3W group. The value of a wrong is not as consistently demonstrated. The values are closer to chance expectation. The values of wrongs are too close to zero to support any conclusion as to positive effect of punishment. The conclusions of Thorndike and of Lorge that learning occurs by virtue of the retention of rewarded responses and not through the elimination of punished responses are substantiated. Even in the favored group (1R 3W) the weighted average value of a single punishment is too close to zero.

TABLE 26

EXPERIMENT 6. GROUP 1R 3W

Code words—Sets *A* and *B*	The summary of the frequencies of $X_aC_sC_s$, X_aC_sA, $C_aX_sX_s$, C_aX_sA, etc. for trials 1, 2, 3, 4, and 5 with subtractions for *a priori* chance			
	No. of responses	Percent all	The excess in percent over chance at the trial for repetition of the same right response	The excess in percent over chance at the trial for a response other than the particular wrong response
At trial 3				
$X_aC_sC_s$	34	40	+15	
X_aC_sA	50	60		
$C_aX_sX_s$	9	21		
C_aX_sA	35	79		+04
At trial 4				
$X_aC_sC_sC_s$	20	59	+34	
$X_aC_sC_sA$	14	41		
$C_aX_sX_sX_s$	2	23		
$C_aX_sX_sA$	7	77		+02
At trial 5				
$X_aC_sC_sC_sC_s$	17	85	+60	
$X_aC_sC_sC_sA$	3	15		
$C_aX_sX_sX_sX_s$	0	00		
$C_aX_sX_sX_sA$	2	100		+25++

TABLE 27

THE EFFICACY OF SUCCESSIVE RIGHTS AND OF SUCCESSIVE WRONGS WHEN CHANCES OF RIGHTS ARE 3 OUT OF 4, 2 OUT OF 4, AND 1 OUT OF 4 RESPECTIVELY (USING ONLY CASES IN WHICH THE RESPONSE AT TRIAL 2 DIFFERED FROM THAT OF TRIAL 1)

Number of successive rights	Group 3R 1W	Group 2R 2W	Group 1R 3W
One	+55	+27	+15
Two	+61	+53	+34
Three	+75++	+64	+60
Number of successive wrongs			
One	−04	−09	−04
Two	−25	−25	+02
Three	−42*	−35	+25++†

* Based on three instances.

† Based on two instances.

The data of Table 27 are in some instances based on very few cases. The average efficacy of a single right and of a single wrong based upon a weighted average, follows:

Group	Weighted average of a single right	Weighted average of a single wrong
3R 1W.....	+32	−09
2R 2W.....	+25	−11
1R 3W.....	+17	+04

References

THORNDIKE, EDWARD L., '32, Fundamentals of Learning. Bureau of Publications, Teachers College, Columbia University, New York.

LORGE, IRVING, '33, The efficacy of intensified reward and of intensified punishment, *Journal of Experimental Psychology*, 1933, **16**, 177–207.

(Manuscript received February 26, 1932)

THE COMPARATIVE STRENGTHENING OF A CONNECTION BY ONE OR MORE OCCURRENCES OF IT IN CASES WHERE THE CONNECTION WAS PUNISHED AND WAS NEITHER PUNISHED NOR REWARDED

BY IRVING LORGE AND EDWARD L. THORNDIKE

Teachers College, Columbia University

Thorndike ['31 and '32] and Lorge ['33a and '33b] have shown that a punished connection is strengthened more by occurring than it is weakened by being punished.

The purpose of the two experiments to be reported here is to determine what difference in strengthening there is between a neutral or ambiguous after-effect and a 'punishment' by the announcement of 'Wrong,' and what differences there are between each of these and a 'reward' by the announcement of 'Right.' In both experiments, the learner responded to a line containing a nonsense word followed by four English words two of which were right, by choosing one of the four. In both experiments each subject had 4 trials with a series of 40 lines, then 4 trials with another series of 40 lines, and so on until twenty series were completed. In both experiments his response was followed by 'Right' + a click, 'Wrong' + a click, or a click alone. That is, not all his right responses were announced as 'Right' and not all his wrong responses were announced as 'Wrong.' Many of each were followed by a click alone. The difference between the experiments was that in Experiment 7 the 'Right' or 'Wrong' was announced after 8 out of 40 lines used, while in Experiment 8 the 'Right' or 'Wrong' was announced after 18 of the 40 lines used.

The instructions to the subjects and the procedure in the experiments were as follows:

"This is an experiment in learning the meanings of nonsense words. You will read each line and underline the word which you think is right, at the same time saying its number

(1 or 2 or 3 or 4) out loud, at the beat of the metronome. Sometimes I shall tell you whether you are right or wrong. Sometimes I shall not. As soon as you finish sheet 1, give it to me and begin sheet 2. As soon as you finished sheet 2 give it to me and begin sheet 3. We will allow one metronome beat for the shift to a new sheet."

We compare the influence of the announcement of 'Wrong' with the influence of the 'click alone' by the percentages of repetitions of the connection in question in the next trial. To avoid the influence of 'spread' from a 'Right' to connections in proximity to it, we compare connections equally remote from a rewarded connection (Thorndike '33a and '33b). We utilize only first occurrences here;[1] so our measurements compare the strengthening over chance (which is .25) due to one occurrence of a connection followed by 'Wrong' (meaning that the connection was wrong), with that due to one occurrence followed by a click alone (meaning that it has equal chances of being right and of being wrong). The facts appear in Table I for Experiment 7 and in Table II for Experiment 8.

Tables I and II show, both for 'Wrong' and for 'click alone' a substantial strengthening (to about 37 percent) over the percentage of repetitions which chance plus the influence of favoritism for certain positions would give. Mere chance would, of course, give 25 percent. To determine approximately what chance plus possible individual tendencies to favor one or another of the four positions would give, we record the number of choices of 1, 2, 3 and 4 made by each subject in each series, omitting the choices followed by 'Right' or 'Wrong.' This gives nearly or quite as good a sampling of each subject's favoritism in choosing numbers as a count of all his choices, and, because of the way in which the primary records were kept, saves considerable time. Each subject's record is inspected to detect any reversals of favoritism in consecutive trials with the same series, any systematic shifts of favoritism, and any clear cases of short-lived habits of favoritism. There are no such reversals, shifts, or short-lived habits of importance. In general the amount of favoritism is

[1] Cases of two and three occurrences of the same connection will be treated later.

TABLE I

THE FREQUENCY WITH WHICH A CONNECTION OF THE STATED DEGREE OF PROXIMITY TO A REWARDED CONNECTION AT ITS FIRST OCCURRENCE WAS REPEATED (*S*) OR REPLACED BY SOME OTHER CONNECTION (*D*) IN THE FOLLOWING TRIAL.

The data are summed for 9 educated adult subjects in Experiment 7.

	Followed by the Announcement 'Wrong'			No Announcement. Click Alone		
	S	*D*	$\frac{100\,S}{S+D}$	*S*	*D*	$\frac{100\,S}{S+D}$
Next to a rewarded connection but not between two	Data too scant					
Two steps from a rewarded connection, but not from two	67	130	34.0	882	1458	37.7
Three steps from a rewarded connection, but not from two	64	105	37.9	603	1046	36.6
Four steps from a rewarded connection, but not from two	76	100	43.2	391	655	37.4
Five steps from a rewarded connection, but not from two	69	122	36.1	311	533	36.8
Six steps from a rewarded connection, but not from two	53	116	31.4	225	402	35.9
Seven or more steps removed from a rewarded connection	156	253	38.1	671	1155	36.7
Sum	485	826	37.0	3083	5249	37.0

such that the probability that, apart from occurrences, rewards, and punishments, the same response would be repeated in two consecutive trials, is not much above the .250 which would result if favoritism were zero. In Experiment 7 this probability, computed from the first two experiments combined, is as follows for each of the nine subjects:—.250, .254, .255, .257, .258, .263, .265, .286, and .295 (Median, .258; Average, .265). Computed from the two successive experiments in which the greatest favoritism appeared it is as follows:—.264, .266, .271, .271, .275, .282, .295, .304, and .359 (Median, .275; Average.288). Computed from the first four experiments combined, it is:—.252, .255, .256, .260, .260, .261, .264, .286, and .290 (Median, .260; Average, .265).

In Experiment 8 it is, when computed from the first two experiments, as follows for each of the ten subjects:—.252, .256, .257, .259, .263, .264, .266, .286, .305, and .330 (Median, .265; Average, .274). Computed from the two successive experiments in which the greatest favoritism appeared, it is:—

TABLE II

THE FREQUENCY WITH WHICH A CONNECTION OF THE STATED DEGREE OF PROXIMITY TO A REWARDED CONNECTION AT ITS FIRST OCCURRENCE WAS REPEATED (*S*) OR REPLACED BY SOME OTHER CONNECTION (*D*) IN THE FOLLOWING TRIAL

The data are summed for 10 educated adult subjects in Experiment 8.

	Followed by the Announcement 'Wrong'			No Announcement. Click Alone			Diff.
	S	*D*	$\frac{100\,S}{S+D}$	*S*	*D*	$\frac{100\,S}{S+D}$	
Next to each of two rewarded connections	51	79	39.2	213	327	39.4	0.2
Next to one rewarded connection	423	704	37.5	1225	2015	37.8	0.3
Two steps from each of two rewarded connections	40	64	38.5	86	182	32.1	−6.4
Two steps from one rewarded connection	277	493	36.0	695	1082	39.1	3.1
Three steps from each of two rewarded connections	28	52	35.0	60	84	41.7	6.7
Three steps from one rewarded connection	201	296	40.5	409	645	38.8	−1.7
Four steps from each of two rewarded connections	15	15	50.0	34	85	28.6	−21.4
Four steps from one rewarded connection	81	159	33.3	199	388	33.9	0.6
Five or six steps from each of two rewarded connections	14	23	37.8	23	59	28.0	−9.8
Five or six steps from one rewarded connection	91	169	35.0	194	356	35.3	0.3
Seven or more steps from each of two rewarded connections	64	91	41.3	132	157	45.7	4.4
Sum	1285	2215	36.7	3270	5380	37.8	1.1
Weighted average difference (Wts. in order 3, 28, 2, 19, 2, 12, 1, 6, 1, 6 and 4							0.5

.261, .262, .267, .282, .287, .295, .296, .301, .302, and .487 (Median, .291; Average, .303). Computed from the first four experiments, it is:—.251, .252, .252, .256, .257, .258, .259, .279, .287, and .304 (Median, .258; Average, .266).

The computations for the sum of the first four experiments give approximate lower limits. The computations for the two consecutive experiments showing the greatest favoritism give results considerably above the upper limits for the person's favoritisms as they operated experiment by experiment.

We may then estimate the probable percentage of repetition in two consecutive trials due to chance and favoritism for certain positions as between 26 and 27 for the group in

Experiment 7 and between 26 and 28 for the group in Experiment 8. $26\frac{1}{2}$ will be very near the truth for both experiments.[2]

There is a substantial strengthening over what chance and favoritism for certain positions would give, by either announcement. The increase is almost the same for one as for the other. If we combine the sums for Table I and II, we have $S = 1770$, $D = 3051$ and $100S/S + D = 36.7$ (an excess of 10.2) for 'Wrong'; and $S = 6353$, $D = 10629$ and $100S/S + D = 37.4$ (an excess of 10.9) for the click alone. The probable error for the percent of repetitions of the wrongs is 0.5; the probable error of the percent of repetition of ambiguous clicks is 0.4. The probable error of the difference between the percents is 0.53, the difference is 0.7. The chances for such a difference are 31 in a hundred. In any separate comparisons where the numbers of cases are large, the differences are small. By any reasonable weighting of the differences the weighted average difference is small. A punished occurrence increases the probability of occurrence in the next trial over nine-tenths as much as an occurrence followed by the click alone.

We may check this conclusion by the facts for the influence of announcements of 'Wrong' after two or three occurrences of a connection compared with the facts for the click alone after two or three occurrences.

In Experiment 7 there was a slight spread of the influence of rewards to connections in close proximity to the rewarded connection. Consequently, it is desirable to classify cases of connections occurring twice or three times according to their proximity to a rewarded connection at each occurrence to make sure that those followed by 'Wrong' are strictly comparable to those followed by only the click. The facts for doubles are shown in Table III. The data for punished connections are too scanty in the first two classes to merit attention. In the last two classes we have differences in favor of

[2] The proper procedure would be to determine this as exactly as possible for each individual in each experiment and to express all results by individuals and in terms of excesses over the probability by chance plus position favoritism for that individual, and then combine these excesses into average excesses for 'Wrong' and for 'Click alone.' But the labor would be enormous and the end result would differ little from that obtained by using $26\frac{1}{2}$ for the summed results.

TABLE III

THE FREQUENCY WITH WHICH A CONNECTION WHICH HAD OCCURRED TWICE WAS REPEATED (*S*) IN THE TRIAL FOLLOWING ITS SECOND OCCURRENCE

Nine subjects in Experiment 7. D = replaced by some other connection. $n = S + D$.

	Consecutive Double Occ.				Non-consecutive Double Occ.			
	Punished by 'Wrong'		Neutral Click Alone		Punished by 'Wrong'		Neutral Click Alone	
	$\frac{100\,S}{S+D}$	n	$\frac{100\,S}{S+D}$	n	$\frac{100\,S}{S+D}$	n	$\frac{100\,S}{S+D}$	n
Next to a rewarded connection at both occ.	—	0	62.9	806	00.0	1	39.5	172
Next to a rewarded connection at one occ.	100.0	1	56.1	433	00.0	1	36.6	113
Within 2 steps of a rewarded connection at one or both occ.*	35.0	60	60.9	891	35.7	14	40.2	189
Not within 2 steps of a rewarded connection at either occ.*	51.2	344	62.4	1463	31.7	82	42.1	355

* The probable errors for the entries in the last two lines of Table III are: for 35.0, 4.2; for 60.9, 1.1; for 35.7, 8.6; for 40.2, 2.4; for 51.2, 1.8; for 62.4, 0.9; for 31.7, 3.5; for 42.1, 1.8.

the neutral after-effect of 25.9, 11.2, 4.5 and 10.4. Using weights roughly as the square roots of the smaller of the population involved in the difference, we have 12.0 as a weighted average difference. The facts for trebles are too scanty to be presented.

In Experiment 8, the spread of influence of the rewards was so slight that proximity to a rewarded connection may be disregarded as a cause of differences between 'Wrong' and 'click alone.' We simply tabulate the facts for two occurrences of a connection according to the proximity of the second occurrence to a reward, and the facts for three occurrences according to the proximity of the third occurrence to a reward. The results appear in Tables IV and V. Two occurrences with 'click alone' show 53.7 percent repetitions in the next trial (or 28.7 over chance). Two with 'Wrong' show 50.3 percent (or 25.3 over chance). The difference (3.4) is 12 percent of the excess for 'click alone.' The facts for three occurrences with 'click alone' and 'Wrong' are 71.9 (or 46.9

TABLE IV

THE FREQUENCY WITH WHICH A CONNECTION WHICH HAD OCCURRED TWICE WAS REPEATED (*S*) OR REPLACED BY SOME OTHER CONNECTION (*D*) IN THE TRIAL FOLLOWING ITS SECOND OCCURRENCE

Data from 10 subjects in Experiment 8.

Degree of Proximity to a Rewarded Connection at the 2d Occurrence	Followed by the Announcement 'Wrong'			No Announcement. Click Alone			Diff.
	S	*D*	$\frac{100\,S}{S+D}$	*S*	*D*	$\frac{100\,S}{S+D}$	
Next to each of two rewarded connections.	30	21	58.8	123	104	54.2	−4.6
Next to one rewarded connection.....	238	227	51.2	710	594	54.5	3.3
Two steps from each of two rewarded connections.....	29	23	44.2	58	45	56.3	12.1
Two steps from one rewarded connection.	140	164	46.1	379	343	52.5	6.4
Three or four steps from each of two rewarded connections.....	21	21	50.0	70	55	56.0	6.0
Three or four steps from one rewarded connection.....	131	132	49.8	334	296	53.0	3.2
Five or more steps from a rewarded connection.....	87	80	52.1	166	147	53.0	0.9
Sum.....	676	668	50.3	1840	1584	53.7	3.4
Probable errors.....			0.9			0.6	1.1

TABLE V

THE FREQUENCY WITH WHICH A CONNECTION WHICH HAD OCCURRED THREE TIMES WAS REPEATED (*S*) OR REPLACED BY SOME OTHER CONNECTION (*D*) IN THE TRIAL FOLLOWING ITS THIRD OCCURRENCE

Data from 10 subjects in Experiment 8.

Degree of Proximity to a Rewarded Connection at the 3d Occurrence	Followed by the Announcement 'Wrong'			No Announcement. Click Alone			Diff.
	S	*D*	$\frac{100\,S}{S+D}$	*S*	*D*	$\frac{100\,S}{S+D}$	
One step removed from a rewarded connection	103	43	70.5	380	137	73.5	3.0
Two steps removed from a rewarded connection.....	54	32	62.8	170	82	67.5	4.7
Three or more steps removed from a rewarded connection.....	75	36	67.6	249	93	72.8	5.2
Sum.....	232	111	67.6	799	312	71.9	4.3
Probable errors.....			1.7			0.9	1.9

over chance) and 67.6 (or 42.6 over chance), the difference (4.3) being 9 per cent of the excess for 'click alone.' For first occurrences in Experiment 8 the corresponding percent was also 9.

The facts for the two experiments may be summarized as follows:—Apart from any influence of occurrences, rewards, punishments and ambiguous clicks, the percentage of repetitions would have been about 26.5. After one occurrence punished by wrong the percentage in the next trial is about 37.0 in Experiment 7 and 36.7 in Experiment 8, averaging 36.8 (an excess of 10.3).

After one occurrence left ambiguous by the click the percentage is 37.0 in 7 and 37.8 in 8, averaging 37.4 (an excess of 10.9).

After one rewarded occurrence it is 59.4 in 7 and 43.8 in 8, averaging 51.6 (an excess of 25.1).

After two occurrences punished by wrong it is 43.5 in 7 and 50.3 in 8, averaging 46.9 (an excess of 20.4).

After two occurrences left ambiguous it is 57.4 in 7 and 53.7 in 8, averaging 55.6 (an excess of 29.1).

After two rewarded occurrences it is 83.6 in 7 and 61.7 in 8, averaging 72.7 (an excess of 46.2).

After three punished occurrences it is 67.6 in Experiment 8 (an excess of 41.1).

After three neutral occurrences it is 71.9 in Experiment 8 (an excess of 45.4).

After three rewarded occurrences it is 94.0 in 7 and 77.5 in 8 (excess of 67.5 and 51.0).

The strengthening by the punished occurrences is thus 95 percent, 70 percent, or 90 percent of that by the neutral occurrences according as we use first, double, or treble occurrences. It is 41 percent, 44 percent or 53 percent of that by the rewarded occurrences, using first, double, and treble occurrences.

There are several possible explanations of the lesser strengthening by a 'Wrong' than by the ambiguous 'click alone.' One is that the strengthening with the click alone is a measure of the force of the one, two, or three occurrences of

the choice in question, and that the punishment by 'Wrong' subtracts from this.

Another explanation accounts for the facts without assuming that the 'Wrong' weakens anything. The click alone permits the learner to retain a belief that his response was right, if for any cause he has such a belief, whereas the 'Wrong' does not. Such an uncontradicted belief may well act to some extent as an actual announcement of 'Right' would act.

A third explanation is that the subjects may occasionally have repeated responses in spite of the precautions taken to prevent their doing so. If they did this at all, they surely would have done it oftener in the case of those called right and those left ambiguous than in the case of those called wrong.

Of these three explanations the second seems to us the most probable. Possibly all three may be true, the effect being shared by the three sorts of cases. In any case, the announcement of 'Right' adds very much more strengthening than the announcement of 'Wrong' subtracts.

References

THORNDIKE, E. L. '31. Human learning.

THORNDIKE, E. L. '32. Fundamentals of learning.

THORNDIKE, E. L. '33a. A proof of the law of effect, *Science*, **77**, 173–175.

THORNDIKE, E. L. '33b. An experimental study of rewards.

LORGE, I. '33a. The efficacy of intensified reward and of intensified punishment. *J. of Exp. Psych.*, 1933, **16**, 177–207.

LORGE, I. '33b. The effect of the initial chances for right responses upon the efficacy of intensified reward and of intensified punishment. *J. of Exp. Psych.*, 1933, **16**, 362–373.

(Manuscript received March 5, 1933)

I. THE INCREASE OF VISUAL ACUITY IN ONE EYE THROUGH ILLUMINATION OF THE OTHER [1]

BY GEORGE W. HARTMANN
Pennsylvania State College

I. The Problem

The Russian physiologist Kravkov (1) has recently reported that an improvement in the visual acuity of the right eye in discriminating black figures on a white ground occurs if the left eye is simultaneously strongly illuminated, but that a deterioration occurs if white objects on a black field are given. The assumption that some kind of central influence had been exerted upon the retinal function seemed probable since consensual pupillary contraction and associated accommodation strain were eliminated by the use of an artificial pupil and a 1 percent solution of homatropin injected into the right eye one hour before the test.

A novel finding such as this deserves to be checked and reinterpreted from the psychological viewpoint because of its obvious bearing upon figure-ground phenomena and Gestalt brain theory. The main purpose of this present paper is to attempt a verification of Kravkov's data by use of a similar but somewhat improved technique. Our problem is simply, What changes in the acuity of the right eye occur when a mixed bright light affects the left retina only?

II. Apparatus

The experimental set-up centers about two tables placed at opposite ends of a nine-meter long dark room—one, the observer's table and the other the experimenter's bench from which the essential stimulating conditions were controlled. The latter will be described first.

A light wooden frame, 69 × 49 cm in size, was clamped to one edge of the experimenter's table. To the front of this frame was fastened some stiff cardboard to which could be attached the large black or white paper that served as 'ground.' Two detachable brass squares, 3 × 3 cm in size, at the end of a small length of sleeve tubing, com-

[1] One of a series of papers supported by the Social Science Research Council.

pletely covered two small holes made in the cardboard through which projected the short bars over which the tubing slid when the squares were fitted flat to the background. Appropriate black or white papers were pasted upon the surface of the brass bases, the distance between which could be adjusted from zero to one centimeter by means of a dial arrangement at the back of the frame. A vernier wheel, calibrated in hundredths, turning along a notched millimeter scale, suspended from a thin steel frame in the rear center of the cardboard, indicated the amount of space showing between the two front squares. Only one square (the one to the left of the observer) was movable, although by induction both squares appeared to be in motion.

To secure even illumination, the room was made light-proof, and the stimulus field illuminated by two perpendicular semi-silvered tubular lamps with 27 cm filaments placed in a rectangular frame about one meter in front of the test squares and so adjusted that no shadows were visible on either the 'figures' or the 'ground.' The only other light sources in the room were a small shielded bulb constantly burning above the measuring scale at the rear of the testing frame sufficing to record the readings of the mechanism, and a strong lamp at the subject's table (see below) for supplying the stimulus to his left eye.

The observer sat at the opposite end of the room with his eye exactly 6 meters from the test squares. A 40-watt frosted Osram lamp was clamped to an upright and bound evenly in a flexible tube of carton and black felt, permitting the exit of a 7 cm circle of light. The subject's left orbit could be pressed firmly but without discomfort against the rim of this tube so that only one retina was directly illuminated. This lamp was governed by a switch at the side of the experimenter's table. The right eye of the observer peered through a 10-diopter lens attached perpendicularly to another upright; and the whole head was supported upon a chin rest to prevent excessive 'wandering' of fixation. The only function of this diminishing lens was to increase the size of the figures from which the limen was computed.[2] An artificial pupil with a diameter of 1.5 mm was placed before the subject's right eye to maintain pupillary influences constant (see Hess (**2**, 177)).

III. Procedure

The solution to our problem lies in a simple comparison of two acuity thresholds for the same right eye—one obtained in normal fashion for the right eye alone without illumination of the left and the other with the lamp affecting one retina. The posture, instructions, and remaining stimuli and conditions affecting the subject were identical in both cases.

Under these circumstances, if the distance between the two squares is zero the subject sees only a smooth regular rectangle on a uniform field. As the experimenter slowly turns the back wheel, the subject watches the appearance of the figure until the first sign of inhomogeneity appears in it. His instructions are to say 'stop' as soon as the first sign of inhomogeneity appears or as soon as the original unity becomes 'twoness.' This is the procedure where the ascending readings are taken. In the descending instances, the subject watches until he can no longer see the split in the middle or until the two squares merge definitely into one.

In alternate cases the light was flashed into the observer's eye simultaneously with a warning signal and followed after a two second adaptation period by the subliminal movement of the squares. The intense and sudden brightening of part of the visual

[2] This 'minimizing lens' was discarded after the data of Table I had been obtained, since the relative thresholds—which alone concern us—were not affected thereby.

field was at first normally experienced as a highly unpleasant shock and some subjects had initial difficulty in holding the eye open or in keeping the head pressed against the light source. But ordinarily a half dozen preliminary and unrecorded practice trials brought the feeling-tone down to neutrality and occasionally, toward the end of the sitting, a mild liking for this odd type of stimulation developed.

Between readings a one minute pause was inserted; longer rest intervals were sometimes given whenever tears arose from strain of fixation or as a protective response to the 'blinding' light. A longer pause of approximately three minutes occurred in the middle of the series when the figure-ground relationship was changed by simply attaching a reserve pair of brass squares with the opposite type of grey paper pasted to their surfaces and unclamping the 'ground' sheet, exposing the other beneath.

A typical test-sequence involved ten ascending and ten descending judgments for each configuration considered. The following schema, in which the numbers indicate the order in which the readings were taken, may make the arrangement clearer:

Figure-Ground	Black on white				White on black			
Stimulation	Normal		Light		Normal		Light	
Direction	Asc.	Desc.	Asc.	Desc.	Asc.	Desc.	Asc.	Desc.
	1	2	3	4	1	2	3	4
	5	6	7	8	5	6	7	8
	etc.							
	37	38	39	40	37	38	39	40
Threshold								

The subjects of the investigation were all graduate students in psychology, between 21 and 35 years of age. Their familiarity with psychological techniques and observations was of value in the phenomenological analysis required. Since the relative thresholds and not absolute efficiency represent the object of interest in this study, no demands for normal vision were made. A few volunteers, however, were rejected because of extreme defects and obvious uncertainty in making judgments.

IV. Results

The first exploratory records were derived from eleven subjects, using the ordinary deep black and clear white cardboard employed in commerce. Table I is of interest because it reveals the tendency which all the more careful later checks only served to confirm.

On the face of these data it would appear that an auxiliary stimulus enhances visual performance in both instances and

TABLE I

COMPARATIVE THRESHOLDS FOR VISUAL ACUITY OF THE RIGHT EYE UNDER DIFFERENT CONDITIONS

The figures under 'light' refer to performance when the left eye is illuminated; the values under 'normal' were obtained when the left eye was free from any visual stimulation.[3]

Observer	Black objects on white ground				White objects on black ground			
	Normal		Light		Normal		Light	
	Mean	A.D.	Mean	A.D.	Mean	A.D.	Mean	A.D.
Wi	3.460	.14	3.193	.16	2.070	.15	1.488	.07
Ca	3.194	.08	3.029	.32	1.583	.09	1.378	.08
Wo	*3.051*	.10	*3.106*	.12	1.779	.13	1.632	.10
Ma	3.625	.20	3.628	.21	2.269	.10	1.959	.11
Fr	2.663	.09	2.368	.09	2.297	.19	2.165	.19
Le	2.531	.13	2.370	.14	1.993	.10	1.879	.08
Ge	2.345	.05	2.286	.13	1.920	.14	1.868	.17
Ju	3.095	.10	2.467	.17	2.374	.30	2.337	.17
vS	2.579	.14	2.515	.09	*2.094*	.09	*2.147*	.15
Ne	2.924	.12	2.777	.12	2.572	.21	2.264	.26
Sz	3.125	.27	3.079	.21	2.367	.23	2.183	.16
Total	2.96	.32	2.80	.37	2.12	.23	1.94	.26
Average difference between means	.16				.18			

[3] All tabular values are millimeter readings. The customary conversion into degrees of visual angle is omitted as unnecessary for the purpose of comparison which is the main concern of this study.

not just in the case of black on white as Kravkov maintains. In general, the striking thing is that not only does no deterioration occur, but improvement in the white-on-black combination is just as great as in the other. The two italicized pairs are the only exceptions to the rule and their differences are much smaller than the average. However, one must withhold acceptance of this result until it has been verified under other and more precise conditions. The slightly greater variability of the 'light' readings implies that they may possess less precision and certainty than the others.

What are some of the objections which may be raised against these tentative findings? Practice has been equalized by the order of stimulation and degree of adaptation similarly does not favor either one or the other mode. Suggestion may

conceivably account for the phenomenon but this possibility was included at the very beginning: Observers Wi and Ca were familiar with the problem, having heard a discussion of the original problem in a colloquium but observers Wo and Ma were wholly ignorant of the 'point' involved, yet there is no real difference between their records; in fact, everything conduces to a negative suggestion, since most subjects protest that they cannot see well if they are to be 'blinded' so severely.

The reasons for the disagreement with Kravkov's data are not easy to locate, since the significant conditions seem identical. He used, to be sure, a rougher measuring device, merely pushing the squares in or out by means of a stiff cardboard lever; and distance effect was secured by a system of mirrors. But unless his findings are an artifact of the kind of paper employed and the nature of the illumination (the light in the present study was strongly yellowish) it is hard to account for the difference.

The next step consisted in seeing if the apparent tendency was operative in other regions of the brightness series. The Zimmermann extremes in the grey scale—the 'Tuchschwarz' and 'Barytweiss' of German visual studies—was employed instead of the ordinary black and white of the first survey. Otherwise, the experimental conditions remained unaltered. Table II presents the data secured by this method from eight subjects and tells much the same story as Table I, except that the absolute differences are now greater and contrary cases do not occur.

A further check was made by using dark, medium, and light grey paper in various combinations as figure and ground. It was supposed that with an increase in brightness similarity a cruder or vanishing effect would be revealed. In order to present as unfavorable circumstances as possible the illumination of the testing area was diminished by stepping down the current supplying the tubular lamps. Table III shows the results obtained with the dark grey and medium grey combination and Table IV presents similar data for the light grey-middle grey pattern.

TABLE II

COMPARATIVE VISUAL THRESHOLDS IN THE CASE OF EXTREME BLACK FIGURES ON AN EXTREME WHITE GROUND AND THE CONVERSE

For conditions see Table I

Observer	Black on white				White on black			
	Normal		Light		Normal		Light	
	Mean	A.D.	Mean	A.D.	Mean	A.D.	Mean	A.D.
Ma	2.42	.13	2.214	.11	2.63	.12	2.58	.30
Ba	2.804	.17	2.516	.12	4.126	.59	3.002	.45
Fo	2.972	.41	2.785	.29	2.744	.13	2.576	.25
Ko	2.883	.26	2.630	.11	3.302	.60	2.981	.43
Mo	2.092	.07	2.002	.04	1.990	.11	1.817	.13
Sc	2.570	.41	2.235	.31	2.415	.35	2.176	.30
Li	3.056	.46	2.304	.25	2.956	.62	2.610	.43
Sn	4.230	.76	3.520	.92	4.188	.35	3.356	.24
Total	2.88	.54	2.53	.34	3.04	.62	2.64	.41
Average difference between means	.35				.40			

TABLE III

COMPARATIVE VISUAL THRESHOLDS IN THE CASE OF DARK GREY FIGURES ON A MEDIUM GREY FIELD AND THE CONVERSE

For conditions see Table I

Observer	Dark grey–medium grey				Medium grey–dark grey			
	Normal		Light		Normal		Light	
	Mean	A.D.	Mean	A.D.	Mean	A.D.	Mean	A.D.
Mc	2.854	.12	2.530	.20	2.104	.18	1.877	.13
Le	6.760	.08	2.726	.40	4.580	.80	3.368	.39
Me	3.484	.35	2.984	.13	2.873	.66	2.679	.44
Be	3.397	.37	3.102	.38	2.648	.60	2.296	.71
Total	4.12	1.32	2.84	.21	3.05	.77	2.56	.47
Average difference between means	1.28				.49			

TABLE IV

COMPARATIVE VISUAL THRESHOLDS IN THE CASE OF LIGHT GREY FIGURES ON A MEDIUM GREY FIELD AND THE CONVERSE

For conditions see Table I

Observer	Light grey–medium grey				Medium grey–light grey			
	Normal		Light		Normal		Light	
	Mean	A.D.	Mean	A.D.	Mean	A.D.	Mean	A.D.
Ma.....	3.490	.23	3.162	.32	3.173	.29	2.679	.20
St.....	3.259	.28	2.949	.23	3.367	.27	2.871	.34
Si.....	2.493	.49	2.413	.51	2.917	.49	2.631	.30
Total.....	3.08	.39	2.84	.29	3.15	.16	2.73	.10
Average difference between means....	.24				.42			

V. PHENOMENOLOGY

Uniformity and consistency are obvious features of the tabular data which point to a rather surprising conclusion. In order to obtain a more complete picture of the entire process, introspective reports were procured from each observer. Spontaneous and accidental remarks made during the sitting also contributed to an interpretation of the conscious experience.

It should be noted that a complete binocular fusion occurs at the very beginning of the session, *i.e.*, the light bulb is not seen separately by the left eye and the testing board by the right—instead, the entire phenomenal field is uniformly of high brightness. The emergence and extinction of the 'split' usually occurred suddenly with both ascending and descending procedures (not so well with the latter) and was very pronounced in the case of black on white. The suddenness with which the threshold is reached argues for the disintegration of one Gestalt and the formation of another, *i.e.*, a Gestalt is dimly known to be forming and when it is mature it 'bursts forth.' Watching for the 'crack' in the case of white on black was less comfortable and the assurance that a correct discrimination had been made was lower.

Most subjects report the following sequence: The light troubles at first but the veil or fog associated with it is soon dispelled. One has the feeling that the beneficial effect of the light is maximal only toward the end of the experimental period when a qualitatively better line is discerned. This qualitative superiority is variously expressed by saying that with the unilateral light the outlines of the squares are sharper and æsthetically more pleasing or that the line interval becomes blacker (or whiter). When the light is switched on the entire room becomes phenomenally brighter and all visual objects—not just the test interval—are clearer. One has the impression of passing from a dark to a light room. It is well-known that a stronger general illumination increases the performance of the eyes in ordinary binocular vision; but it is odd to find the same outcome under very special monocular conditions. The auxiliary stimulus appears to confer supraliminal status upon an originally subliminal process. This was very simply checked at the end of each session by presenting again to the subject his lowest reading for the 'normal' arrangement. When asked to describe what he saw without the application of light the crack was generally reported as 'shady'; but as soon as light struck the left eye the resulting superior definition provoked comment. In some instances, the squares were believed to have come nearer to the subject!

In the main, the introspective protocols confirm the objective findings and reveal an identity of effect in the cases of black on white or white on black. White on black irradiates much more than the reverse grouping and this probably accounts for the lower degree of satisfaction in seeing almost unanimously reported in that instance. But the auxiliary light stimulus has the same positive effect in both configurations.

VI. Conclusions

Since the early researches of Aubert it has been known that the processes in both retinæ will unite to form one 'total sensation' provided that the two processes do not differ significantly from each other. However, it is doubtful if binocular fusion as ordinarily understood provides an adequate

theoretical basis for the phenomena here described. More to the point are the rare studies on the binocular effects of monocular stimuli as studied by Fechner, Béclard, and Titchener (**3**), particularly with reference to colored after-images. If one eye be closed while the other fixates a red circle and then the fixating eye shut and the previously closed eye opened, the latter will observe a weak greenish aureole on an appropriate ground. The course of the after-image in the indirectly influenced eye is characteristically different from that in the one directly affected, but the presence of cross-currents of some kind seems plain.

How is the facilitating effect of light in the left eye upon performance in the right to be explained? The changes are small and the degree of improvement effected is hardly large enough to have any practical meaning although it is curious that one could temporarily make a myopic eye functionally emmetropic by this method! The fact itself is clear enough but it is difficult to conceive the nature of the mechanism through which this is accomplished. It may be that the total organism is brought to a higher level of readiness or achievement through the tonic influence of the auxiliary stimulus—in which case the present problem would be brought into relation with the poorly understood facts of dynamogenesis. That the attention factor as such is functioning more efficiently is unlikely since motivation is certainly not so one-sidedly distributed in the experimental situation. The more subtle objection that the limen is more quickly reached in the case of the light because its uncomfortable radiance offers an inducement to speedy location of the threshold is needlessly acute and easily shown to be false by appeal to the *descending* threshold where the observer voluntarily prolongs his assumed distress!

It may seem futile to offer a theoretical account which is as removed from experimental attack as the hypothesis about to be developed; nevertheless it is presented as a possible interpretation of the facts. There is no uniform reason for the general superiority of binocular acuity to monocular, but the following explanations have been advanced: better fixation, widening of the pupil in the open eye when the other is closed, summation

of stimuli. It is unlikely that the experimental situation employed in this study aided fixation in any way and the artificial pupil certainly eliminated the influence of contraction or dilation. There remains, then, only the factor of summation, which in this case does not require foveal stimulation of both retinæ by the same object—peripheral excitation of one may suffice, provided the fovea of the other is affected. The larger the number of brain cells aroused in the cortical projection area the finer will be the discrimination mediated by any one region. The sensitization of the entire optic sector increases the efficiency of a part just as the ripples formed by the wind on the surface of a pond merge locally with those produced by a stone thrown into the water. The greater energy of the brain process caused indirectly by stimulation of the 'unused' eye has the same effect as direct local increase in the illumination of the testing squares. Some such central theory would appear to be indicated since it is less of a *tour de force* than the assumption of an interchange of energy between the retinæ themselves.

Summary

In opposition to an earlier experiment by Kravkov, this study indicates an increase in the visual acuity of the right eye for *both* black objects on a white ground and white figures on a black field when the left retina is simultaneously strongly illuminated. Configurations with different types of greys pointed in the same direction. A brain theory based upon summation of stimuli is developed to account for these findings.

References

1. Kravkov, S. W., Ueber eine zentrale Beeinflussung der Sehschärfe, *Graefes Arch. f. Ophthal.*, 1930, **124**, 76–86.
2. Hess, C. V., Pupille, *Bethes Handbuch d. norm. u. pathol. Physiol.*, 1931, 12/1, 176–186.
3. Titchener, E. B., Ueber binoculare Wirkungen monocularer Reize, *Phil. Stud.*, 1893, **8**, 231–310.

(Manuscript received February 29, 1932)

II. CHANGES IN VISUAL ACUITY THROUGH SIMULTANEOUS STIMULATION OF OTHER SENSE ORGANS [1]

BY GEORGE W. HARTMANN

Pennsylvania State College

I. Nature of the Problem

The doctrine of *Gestalt* has led in recent years to new and vigorous attacks upon the problems of perceptual performance. Most of these studies have confined themselves to an examination of the effects of limited changes in the organization of a single sensory field upon the observer's response. But there is nothing in the logic of configurational thinking to forbid an extension of this type of reasoning to inter-sensory influences. Just as our knowledge of the nature of intelligent conduct has been enlarged by the innumerable correlational researches involving it, so one may reasonably hope for a better comprehension of visual phenomena when their reciprocal relations with auditory, olfactory, and the remaining receptors have been determined. The present paper represents a minor effort in this direction.

Urbantschitsch (**7**) in 1880 reported some observations indicating that the sensitivity of the eye to light could be enhanced by simultaneous excitation of the ear, especially if the tube were filled with wax. The theoretical implications of this finding were not made at the time, presumably because the requisite interpretative background was absent. More recently, Lasareff and particularly Kravkov (**6**) have unearthed new evidences of intersensory relationships which were formerly thought to be restricted to the oral cavity with its union of olfactory and gustatory elements. Using a telephone tied up with a low frequency cathode tube generator, Kravkov found that a tone of 2100 vibrations per second increased the

[1] One of a series of studies made while the author was a foreign fellow of the Social Science Research Council of New York.

visual acuity of nine out of ten subjects tested with black figures on a white ground. Even more curious is his further claim that the acuity is lowered under the same conditions when white figures on a black ground are to be discriminated. The present study takes Kravkov's work as its point of departure and attempts an extension of its methodology to other fields. What, then, is the effect of simultaneous auditory (olfactory, tactual, pain) stimuli upon visual performance?

II. Apparatus and Material

Unless otherwise stated, the visual testing material employed in this experiment is identical with that used in a previous paper on "The Increase in Visual Acuity of One Eye through Illumination of the Other" (3) to which the reader is referred for basic details, particularly for a description of the testing board and the methods of operating it. In the present report, however, sound, smell, and cutaneous stimuli take the place of the auxiliary light stimulus applied in the earlier study. The equipment required during the course of the experiment is described under the appropriate sense modality below.

A. Sound.—An adjustable Edelmann pipe was clamped vertically to an upright placed upon the observer's table in such a way that while his right eye was fixating the testing field the sound waves emerged from the tube about 10 cm from his left ear. Two calibrated pipes were used—one emitting a sharp, high tone of 2100 vibrations and the other a deep, low tone of 180 vibrations. They were actuated by a steady blast of air driven through large sheet-metal piping by a motor in an adjacent room. A wooden lever-type sound valve with a feather tension-spring, placed at the end of the piping, could be opened by pulling on a string leading to the experimenter's table over a series of pulleys; when released, it closed of its own accord by dropping back into position. Thin rubber tubing united the exit hole of the sound valve with the base of the Edelmann pipe. In this way the application of the auditory stimuli was easily controlled.

B. Smell.—As in the case of the tones, two kinds of olfactory agents were chosen—citronellol and xylenol crystals (1 : 4 : 5), both in solution. Since the application of the odors demanded a number of active movements on the part of the observer—in distinction from the muscular passivity characteristic of the other situations—a bottle of tap water was introduced as a control.

C. Contact.—The pressure stimulus was applied by means of the following device, the baseboard of which was clamped to the subject's table: A short thin bar with a 2 cm projection serving as a blunt hammer-head was poised upon a metal support at a level 4 cm above the baseboard. Immediately in front of the post were two magnetic spools which pulled the bar downward whenever a current was passed through them. As soon as the circuit was broken the hammer-head mechanically returned to its original position by means of a small counter-weight at the opposite end of the bar. An ordinary Morse key at the experimenter's table controlled the pressure apparatus by means of suitable wiring leading through a storage battery. The subject's left hand rested palm downward upon the baseboard in such a way that the contact or pressure was applied to the skin on the back of the hand about 2 cm below the middle knuckle; the surface affected was approximately 1 cm in radius. Adjustments dictated by the

size of the hand could be made by shifting the counter-weight or raising the supporting post.

D. Pain.—The pain stimulus was applied by means of a very slight modification of the contact apparatus described in the preceding paragraph. A hole was drilled in the center of the hammer-head and an ordinary stiff pin thrust through it. The skin region affected was roughly the same as in the foregoing instance. The penetration of the point was deep enough to yield a genuine pain sensation but not intense enough to provoke withdrawal.

III. General Procedure

The process of measuring the acuity of the right eye which is the heart of the method here involved occurred under the following conditions: The observer was comfortably seated at his station and fixated the testing field with his right eye; during the entire sitting the left eye was covered with a black eye flap. The subject always knew in advance what kind of auxiliary stimulus would affect him but was told to concentrate his attention upon the task of discriminating the squares and to disregard as far as possible the concomitant influences. When readings were made the observer was always given some preliminary practice to be sure he understood his task and discharged it adequately. In both the normal and critical series (see the tables below) a warning signal preceded each observation so that the subject's eye was accommodated before the experimenter introduced any change in the external situation. Unless otherwise stated, thresholds were determined by the method of limits, alternating normal and crucial readings and ascending and descending movements.

A. Auditory Procedure.—Throughout the sound period the low steady hum of the driving motor from the adjacent room was audible in the experimental chamber, but since it represented a constant factor in the comparisons it was disregarded in the interpretation because all the sounds were projected upon it as a common 'ground.' When the situation had been prepared as described in the preceding paragraph, the experimenter cried, 'Ready,' paused two seconds, and then turned the wheel, halting as soon as the observer shouted, 'Stop.' This was all that occurred when 'normal' readings were taken. In the case of the 'critical' determinations, the experimenter pulled the string lifting the sound valve with one hand at the same time that he began to turn the spacing wheel; both actions were likewise simultaneously terminated as soon as the signal to cease was given. In this way perfect parallelism between the main and auxiliary stimulus was preserved. It should be mentioned that the records for the high and low tones were obtained in two different sittings on successive days in order to avoid fatigue.

B. Olfactory Procedure.—The application of the smell stimuli was rather awkwardly made by the observer himself in time with the experimenter's signals. When the fixation posture had been adopted the experimenter said, 'Ready,' waited two seconds while the subject, maintaining his adjustment, removed the stopper from the appropriate bottle and brought the mouth of it in contact with his nostrils, and then exclaimed, 'Now' at which moment the observer began to inhale and the squares were set in motion. This continued until the limen was discerned and the bottle returned to its place in the rack. The citronellol bottle was used in one series alternately with the water as a control for the 'normal' reading in order to preserve equivalence of perceptual and muscular action; and similarly for the xylenol bottle.

C. Tactual Procedure.—The main problem in this instance was the comfortable adjustment of the subject's left hand to the pressure apparatus until a definite sensation of weight was reported. Awareness of the contact area was of course maximal at the

moment the hammer fell, but after a few seconds of immobility it had almost vanished, since the weight was comparatively light.

D. Pain Procedure.—This was practically identical with the foregoing arrangement except that there was grave danger of making the algetic effect too intense with the result that the hand would be reflexly withdrawn. Since the degree of pain which can be quietly borne varies markedly from person to person, identity of external stimulation probably did not exist for all subjects, although of course it remained constant for each individual record. To a somewhat lesser degree this same consideration applies to the tactile stimulus.

IV. Results and Discussion

Table I gives the data derived from the first group of ten subjects in the auditory set-up.

Taking these figures at their fact value it would appear that both tonal stimuli increased apparent visual acuity, irrespective of the nature of the pattern presented. This partly confirms the earlier work of Kravkov who found that a relatively high tone enhanced discrimination in the case of black on white—but it also is opposed to his other finding that a *deterioration* ensues under the same conditions when white on black is involved. Not only is the effect uniformly positive; it seems tentatively to be independent of the pitch quality of the auxiliary stimulus.

Table II presents the records obtained where odors served as the auxiliary stimuli.

In Table III appear the data from two observers in the pressure tests.

In Table IV will be found the results secured from two further subjects under the application of coincident pain.

The most casual inspection of Tables II, III, and IV shows that they all conform to the pattern established in Table I. There is a definite and consistent improvement in visual performance in every instance where auxiliary excitation occurs. The degree of improvement, to be sure, hinges somewhat upon the nature of the secondary stimulus involved, being greatest in the case of sound and smell and least with the cutaneous senses. It is conceivable that the beneficial effect is proportional to the intensity of the auxiliary agent—a view which could be adequately tested within any one mode, but is at present hardly amenable to inter-sensory comparison.

TABLE I

Visual Acuity of the Right Eye under Different Conditions

'Figure' and 'ground' were alternately represented by Zimmermann's 'Tuchschwarz' and 'Barytweiss.' 'Normal' means a reading obtained without the auxiliary sound stimulus

Observer	High tone (2100 vibrations)								Observer	Low tone (180 vibrations)							
	Black/white				White/black					Black/white				White/black			
	Normal		Tone		Normal		Tone			Normal		Tone		Normal		Tone	
	Mean	A.D.	Mean	A.D.	Mean	A.D.	Mean	A.D.		Mean	A.D.	Mean	A.D.	Mean	A.D.	Mean	A.D.
Ad.	2.467	.24	2.287	.31	2.700	.39	2.547	.34	Wol. . . .	2.020	.28	1.764	.25	2.240	.30	1.874	.24
Mac.	1.848	.21	1.744	.23	1.996	.17	1.899	.19	Me. . . .	2.247	.50	2.006	.22	2.467	.57	2.078	.53
Cal.	2.106	.10	1.981	.13	1.959	.19	1.796	.13	Jur. . . .	2.258	.44	2.078	.40	2.554	.29	2.276	.24
Kos.	2.122	.33	2.049	.32	2.259	.54	2.193	.75	For. . . .	2.444	.37	2.246	.29	2.654	.42	2.520	.57
Ba.	2.440	.52	2.190	.52	3.579	.87	3.121	.54	Sch. . . .	2.034	.15	1.893	.26	2.201	.34	2.034	.41
Kr.	2.765	.47	2.324	.39	2.610	.30	2.358	.21	Sch. . . .	2.391	.41	2.178	.36	2.594	.19	2.389	.24
Wi.	2.395	.34	2.122	.24	2.784	.49	2.379	.47	Nes. . . .	1.906	.28	1.689	.21	1.856	.24	1.756	.27
Ma.	2.057	.29	1.941	.27	2.440	.44	2.302	.42	Ber. . . .	1.835	.20	1.736	.20	2.431	.26	2.350	.24
Eln.	1.667	.07	1.604	.04	1.543	.05	1.488	.07	Eln. . . .	1.756	.18	1.597	.06	1.707	.12	1.604	.11
Gwh.	2.126	.11	1.879	.17	2.720	.22	2.428	.14	Gwh. . .	2.109	.20	1.906	.16	2.245	.22	1.925	.15
Total.	2.199	.24	2.012	.18	2.459	.42	2.251	.25		2.100	.19	1.909	.17	2.294	.24	2.081	.24
Average difference between means. . .	.187				.207					.181				.213			

TABLE II

EFFECT UPON THE VISUAL LIMEN OF OLFACTORY STIMULI

For general conditions see Table I (caption)

Observer	Pleasant odor (citronellol)								Observer	Unpleasant odor (xylenol)							
	Black/white				White/black					Black/white				White/black			
	Control		Smell		Control		Smell			Control		Smell		Control		Smell	
	Mean	A.D.	Mean	A.D.	Mean	A.D.	Mean	A.D.		Mean	A.D.	Mean	A.D.	Mean	A.D.	Mean	A.D.
Fr.	2.72	1.29	2.43	.87	2.90	1.34	2.50	1.30	Mo.	2.19	.11	1.83	.18	2.69	.27	2.39	.21
Sc.	2.46	.40	1.99	.25	—	—	—	—	Dr.	2.08	.13	1.89	.22	2.23	.25	1.97	.38
Vs.	2.21	.19	1.90	.25	2.37	.25	2.17	.34	Ma.	2.25	.69	1.81	.32	2.88	.35	2.44	.60
No.	2.34	.20	2.03	.12	3.28	.27	2.76	.27									
Ca.	1.69	.17	1.48	.19	1.74	.10	1.60	.33									
Total.	2.28	.27	1.86	.26	2.57	.52	2.26	.37		2.17	.06	1.84	.03	2.60	.25	2.27	.20
Average difference between means...	.42				.31					.34				.33			

TABLE III

EFFECT OF AN AUXILIARY CONTACT STIMULUS UPON THE VISUAL THRESHOLD

For the general conditions see the caption of Table I

Observer	Black on white				White on black			
	Control		Pressure		Control		Pressure	
	Mean	A.D.	Mean	A.D.	Mean	A.D.	Mean	A.D.
Vr.....	1.92	.37	1.84	.35	2.74	.33	2.62	.42
Ca.....	1.93	.16	1.90	.17	1.96	.24	1.86	.21
Total.....	1.93	.01	1.87	.03	2.35	.39	2.24	.38
Average difference between means.....	.06				.11			

The consequences implicit in the above data are sufficiently startling to raise the question of the measure of confidence which can be placed in them. To the writer there remained the deadly suspicion that the uniformity of result shown by the records might be a sheer artifact of the technique employed. It will be remembered that procedure used so far embodies the traditional psychological method of limits, one of the most convenient technical devices of the laboratory. In this study, the wheel governing the size of the space lying

TABLE IV

INFLUENCE OF SIMULTANEOUS PAINFUL STIMULATION UPON THE VISUAL ACUITY OF ONE EYE

For general conditions see heading of Table I

Observer	Black on white				White on black			
	Control		Pain		Control		Pain	
	Mean	A.D.	Mean	A.D.	Mean	A.D.	Mean	A.D.
Le.....	1.73	.08	1.69	.12	1.54	.10	1.44	.08
Ge.....	2.05	.29	1.88	.27	2.26	.26	2.13	.25
Total.....	1.89	.16	1.79	.10	1.90	.36	1.79	.34
Average difference between means.....	.10				.11			

between the test squares was turned steadily in either the ascending or descending direction by the experimenter, who presumably might be influenced in his turning rate or stopping speed by his unwitting preferences in the comparison. This objection, however, is hardly valid since the dial was manipulated with averted or closed eyes so that the experimenter could scarcely have been affected by an unknown (or at least only approximately known) point on the scale. Even the variability in turning rate which undoubtedly existed may be construed as an asset since it forbade the observer from using a too-constant time factor as a secondary cue. To add certainty to these claims, a second experimenter, wholly uninformed as to the 'point' of the investigation acted as substitute on several occasions and obtained results in no way essentially different from those secured by the writer. Nevertheless, it was felt that an independent confirmation by means of another accepted technique would be desirable, so that the method of constant stimuli was appealed to in order to provide a check on the earlier method (see Kellogg (**5**)).

The procedure employed with this new method involved repeated observations at various scalar points slightly above and below the limen. The experimenter merely presented fixed intervals on the testing board to the subject who was also simultaneously exposed for a definite period, usually ten seconds, to the auxiliary stimulus. At the end of each observation, the subject rendered his judgment on the appearance of the 'crack' according to the following key:

$+$ = clearly and distinctly seen
$\pm$ = dimly but steadily visible
$-$ = uncertain of presence, fluctuating.

The observers were cautioned to adhere to their original criteria in order to ensure comparability of the data, and after a little preliminary practice most of them reported a high degree of confidence in their utterances. Where uncertainty did appear it occurred just where one would expect it to emerge—in the region of the threshold.

For the purposes of this investigation, the simplest possible

scoring method which would ensure comparison was adopted. A + judgment was arbitrarily assigned a value of 3, a ± judgment a weight of 2, and a − judgment a value of 1. These values were then summed and divided by the number of judgments made, which varied somewhat from person to person, although a minimum of one hundred double records—*i.e.*, paired readings for the critical and control situation—was always taken. Table V presents the mean values thus derived.

TABLE V

EFFECT OF AN AUXILIARY TONAL STIMULUS (2100 VIBRATIONS) UPON VISUAL ACUITY AS MEASURED BY THE METHOD OF CONSTANT STIMULI

Observer	No. of judgments	Black on white		White on black	
		Normal mean	Critical mean	Normal mean	Critical mean
Ju	120	1.52	1.90		
Ca	112	1.90	1.98		
Ko	122	2.07	2.11		
Sc	110	1.95	2.00		
Si	100	1.84	2.46		
Li	114	1.49	1.70		
Sh	140			1.49	1.63
Mu	102			1.94	2.22
Kr	154			2.12	2.30
Ha	176			1.90	1.90
Le	108			2.10	2.10
Ge	118			2.22	2.31
Total means		1.79 (.19)	2.02 (.17)	1.96 (.18)	2.07 (.21)
Average difference between means		.23		.11	

For external reasons, the further checking of the earlier findings by this method was interrupted, but from Table V it will be seen that the two techniques point in the same direction; and there is no reason for believing that this trend would be different in the case of the other auxiliary stimuli. Consequently, even though one may be unwilling to assert that a positive improvement in visual performance occurs with *all* secondary stimulation just because the differences are small, it does seem that more evidence for that point of view is available than for any other.

V. Phenomenology

Brief introspective protocols were obtained during the course of the experiment in the hope that some clue to the brain processes involved would be forthcoming. A few remarks based upon the subjects' reports immediately following stimulation will be offered under the appropriate sensory heading.

A. Sound.—For most observers the tone was perceived as part of the total 'ground' and rarely localized near the ear. One subject maintained, "There is no doubt that I was more tense during the sound period. Perhaps that is why the experience is mildly disagreeable even at the start. There are two things I seem to notice: In certain cases where the tone ceased, the split also vanished definitely; but there were other instances in which I saw the limen for a moment after the sound had been given, following which it vanished and was just as invisible as before." Another observer claimed that his concentration upon the field was better when the auxiliary stimulus was active. Still another insisted that he worked better without the 'distraction'!—despite the fact that the objective record indicated that he was genuinely benefitted thereby! One subject picturesquely declared that the tone "jarred the two squares apart so that a better-defined crevice appeared in the center." Another had the impression that the liminal area was agitated or in motion during the application of the sound in contrast to the calm which preceded.

B. Odor.—Long ago the fable of Condillac's statue gave smell a novel position among man's sensory receptors and some recent experimental work suggests that retentive behavior is strongly influenced by it (1). The verbal statements of our subjects unfortunately give no hint concerning the manner in which improvement is effected. The only noteworthy report is merely another instance showing how poorly most observers gauge the efficiency of their own performance: "This odor (xylenol) is so vile that I am sure one of the recorded series has lower values just because of that disagreeableness; if the badness of the smell isn't the cause, then its penetrating and consequently more distracting character must influence the work detrimentally."

C. Touch.—This particular form of cutaneous stimulation was undoubtedly mixed, *i.e.*, contact, pressure, and temperature (cold) elements were all combined in one physical agent. The subjects were able to discriminate two phases in the process: 1, a dynamic stage identified with the immediate striking of the hammer and accompanied by a sharp but momentary brightening of the field of view; and 2, a static period when the hammerhead surface was at rest and a diminution of the original effect had occurred. The general influence of the stimulus was thought to be very weak.

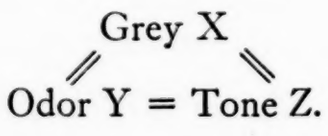
D. Pain.—The idea of using pain as one of the secondary stimuli arose in a purely fortuitous manner—one of the observers who had just completed a tonal series was idly twirling a scarf pin and accidentally stuck his flesh. The mild fright or bewilderment which ensued interfered with accurate observation but he maintained that the small objects he was fixating at the time stood out more prominently. It may be that this is the origin of the popular custom of pinching one's self to enhance a conviction of external or personal reality! In its functioning the pain factor was very similar to the pressure stimulus except that the difference between the static and dynamic phases was less noticeable; in other words, the experience possessed a fairly constant intensity from the moment of application to the moment of termination.

In general, it is clear that the same or approximately the same visual effect is secured through a variety of channels. Externally, it brings to mind Johannes Mueller's favorite illustration by which he was led to the doctrine of specific energies—the fact that the optic nerve yielded only sensations of light, regardless of whether it had been aroused by luminous, mechanical, electrical, chemical, or circulatory stimuli.

VI. Conclusions

During the past decade, Hornbostel (4) has carried on theoretical and experimental investigations on the so-called 'unity of the senses,' the main outcome of which has been the discovery of a common suprasensory factor known as 'brightness.' It is the characteristic shared by most high-pitched

tones, 'loud' colors, penetrating but pleasant odors, sharp 'pointed' tactual stimuli as opposed to dull blunt surfaces, etc. The strongest evidence in favor of its existence is the curious triangular equation easily obtained from large groups. If one produces upon the color wheel greys resulting from five different proportions of black and white and presents with each a definite tone, there is considerable agreement that one and only one of the greys has a 'brightness value' most like the sound. Unanimity, of course, does not occur but even the most confirmed sceptic is surprised to find the 'voting' more uniform than chance would allow. The really startling thing, however, develops when one finds that a given odor can similarly be equated with the preceding grey, and that this can be indirectly checked by equating the odor with the tone originally used. Things equal to the same thing are equal to each other. The situation may be represented as follows:



The pertinence of this to the present study lies in the possibility which it suggests of one sensory agent serving as a surrogate for another. The implications this has for the problem of synaesthesia are obvious and will not be developed here. Apparently, lights, sounds, smells, pressures, and pains do have some property or properties in common, for how otherwise would one account for their similar influence upon visual acuity?

It must be confessed, however, that an imperfection in the symmetry demanded by the Hornbostel schema has been unearthed, since the thought back of the double tonal and olfactory stimuli ran somewhat as follows: If Kravkov found that a high (*i.e.*, bright) tone increased visual efficiency in the case of black squares on a white ground but decreased it in the case of white objects on a black field, then a low (*i.e.*, dull) tone should have just the opposite effects. A kindred effect was anticipated in the use of the 'bright' and the 'dull' smells. Unfortunately, we were able to confirm only part of Kravkov's

original research. The facilitating effects of the auxiliary stimuli are approximately the same in both the black-white and white-black configurations; moreover, the influence of stimuli low in 'brightness value' is in no way different from that of stimuli high in this attribute. One is therefore compelled to seek an explanation which will do justice to a varied set of complex facts and yet make some use of the ingenious theory already available.

Before attempting to sketch the outlines of a new hypothesis, it should be understood that not all auxiliary stimuli have facilitating effects. Auditory localization, *e.g.*, according to the report of Goldstein and Rosenthal-Veit (**2**) is less accurate when manual pointing to the source direction is made with open eyes than with closed. Furthermore, a sound can be displaced toward the right from a subjective center by an eye movement toward the left even though the head be kept stationary.

The writer assumes that the activity of any one sense organ does not remain confined there but tends to affect, although in lesser degree, the general plane of excitation of the sensorium as a whole. To take a specific case: It is well-known that a sudden loud noise will enhance via the vestibule the tonicity of the grosser bodily musculature. If the energy can pass into the massive organic tissues along this path, why should it not also be capable of discharging into the ciliary and thereby affect, be it ever so slightly, the accommodation mechanism? Something like this unquestionably occurs but it would not account for the experimental results under discussion because the artificial pupil excludes a peripheral explanation. One is therefore thrown back upon the brain process as such. There one may suppose that the temporal region is not the only locus of cortical response evoked by an auditory stimulus, but that the remaining brain areas are also aroused, probably through the medium of the so-called association fibres. If, as in this study, the occipital lobe is the primary 'seat' of excitation and the temporal region is also simultaneously thrown into action, a reciprocal exchange of energy occurs in order to preserve neural equilibrium. This would demand that auditory acuity be increased when auxiliary visual stimulation is presented—a

consequence which seems probable if the data of this paper be valid. An investigation is at present in progress to test this assumption of interchange.

But how does this reciprocal flow of energy—which rings very much like the drainage hypothesis except that the flow is in both directions—result in superior visual performance? If acuity be in part a function of the number of brain cells aroused (or the intensity with which they are aroused) then the excitatory waves coming from some other receptors may be supposed to overflow into the calcarine sector and either throw into action more cortical units or reënforce the excitation in those already active; in either case visual discrimination would be heightened. Where this contributory energy comes from is a matter of indifference for the effect is to a great extent independent of the source and quality of the stimulating agent.

Summary

The paper reports evidence indicating that visual acuity can temporarily be increased by the simultaneous application of auditory, olfactory, and cutaneous stimuli. High and low tones, pleasant and unpleasant odors, mild tactile and pronouncedly painful stimuli, all enhance the discrimination of black-on-white or white-on-black configurations. Checking the records obtained with the method of limits by the method of constant stimuli made no alteration in the findings. A neurological explanation is attempted in terms of a dual interchange of energy.

References

1. Frank, J. D., Affective value vs. nature of odors in relation to reproduction *Am. J. Psychol.*, 1931, **43**, 479–483.
2. Goldstein, K. and Rosenthal-Veit, O., Ueber akustische Lokalisation und deren Beeinflussbarkeit durch andere Sinnesreize, *Psychol. Forsch.*, 1926, **8**, 318–335.
3. Hartmann, G. W., Increase in visual acuity of one eye through illumination of the other, *J. Exper. Psychol.*, June, 1933.
4. Hornbostel, E. M. v., Die Einheit der Sinne, *Melos*, 1925, **4**, 290–297.
5. Kellogg, W. N., An experimental comparison of psychophysical methods, *Archives of Psychol.*, 1929, No. 106.
6. Kravkov, S. W., Ueber die Abhängigkeit der Sehschärfe vom Schallreiz, *Arch. f. Ophthalmologie*, 1930, **124**, 334–338.
7. Urbantschitsch, V., Ueber den Einfluss einer Sinneserregung auf die übrigen Sinnesempfindungen, *Arch. f. d. ges. Physiol.*, 1880, **42**, 155 ff.

(Manuscript received February 28, 1932)

Appendix

After the completion of the foregoing manuscript there came to the writer's attention a valuable related experiment by K. Zietz, a pupil of Werner's at Hamburg, entitled: Gegenseitige Beeinflussung von Farb- und Tonerlebnissen: Studien über experimentell erzeugte Synästhesie, in the *Zeitschrift für Psychologie*, 1931, **121**, 257–356. Zietz reports that an auxiliary sound exerted a powerful influence upon the course of an after-image, causing it to flicker when the tone 'vibrated' intermittently, and extinguishing the image when it ceased. When the auxiliary tone had a vibration number of 200 the color was darker, warmer, softer, duller; the contours also tended to vanish. With a tone of 550, the color was brighter, colder, clearer, harder; and the contours were sharper. Occasionally, under the influence of a high tone (1100 vibrations) a rounded after-image took on a squarish form! As far as the reversal of effect was concerned, it appeared that a tone sounded in a lighted room was judged higher than one acting in the dark.

There is ample reason for believing that Zietz's work supports the main contention of the present account. Its great value in this connection lies in the indication that intersensory factors influence not only acuity but also form and color value.

Another recent contribution to the young literature on "intersensory induction" has been made by von Schiller (Das optische Verschmelzen in seiner Abhängigkeit von heteromodaler Reizung, *Zsch. f. Psychol.*, 1932, *125*, 249–288). In situations involving "heteromodal" stimulation, he found that "pulsation" or "coarseness" may be considered along with Hornbostel's "brightness" as another universal intersensory moment. Using the threshold of fusion with black-white discs as his "base," he observed that beats from an auditory source strengthen and emphasize the flicker effect, while dichords in firsts and fifths (as well as the simultaneous finger "feeling" or exploration of smooth cardboard) minimize it. The prominence of the flicker increased as the accompanying beats rose from four to twelve per second, but at twenty a better fusion occurred.

INTENSITY OF LIGHT AND AREA OF ILLUMINATED FIELD AS INTERACTING FACTORS IN SIZE OF PUPIL

BY C. E. FERREE, G. RAND AND E. T. HARRIS*

The size of the pupil is known to be affected by such factors as intensity of light, state of accommodation and convergence, age, errors of refraction, drugs, emotional states, etc. It is the purpose of the present paper to show the influence of the size of the illuminated field or the size of the stimulus as a factor influencing size of pupil.

The effect of light on the reaction of the pupil would be expected to sustain a fairly close relation to the effect on visual sensation. If area or size of stimulating surface has an effect on the intensity of the sensation aroused it would be expected to have a corresponding effect also on the amount of reaction of the pupil.

Three possibilities may be noted as to the effect of increase in the size of the stimulus on the response of a sense organ. (1) There is an increase in the total energy of excitation in direct proportion to the increase in the size of the stimulus. If it be conceived that the receptors act collectively one might expect an increase in the response of the sense organ in direct proportion to the increase in the area of the stimulus, provided that the receptors included in the area stimulated are equally sensitive. (2) If on the other hand it be conceived that the receptors act independently it might be expected that the intensity per unit area of stimulus, not the total intensity, would be the effective factor in determining the response of the sense organ. In that case increase of area would not increase the power of the stimulus to arouse a response in the sense organ. (3) A third view would be that increase of area is effective in arousing the response of the sense organ but that

* From the Research Laboratory of Physiological Optics, Wilmer Ophthalmological Institute, Johns Hopkins Medical School.

the effect is not directly proportional to the increase of area. In a somewhat extensive investigation of the response of the retina made in a number of studies, we have found this latter view to be correct within limits. That is, within a certain range of change, an increase of size of stimulus increases the response of the sense organ, but apparently not in direct proportion to the change in size. Even if the first view as stated above, correctly represents the law of response of nervous structures, the conditions for its fulfillment would not be found in the retina. That is, it presupposes a uniform distribution of sensitivity over the receiving surface. Uniform sensitivity occurs over very limited areas in the retina.

In direct contradiction to the supposition that the effect of light on the reaction of the pupil would be expected to sustain a fairly close relation to the effect on visual sensation, is a statement which occurs in Fuchs' Textbook of Ophthalmology (Fifth Edition, p. 382). The statement is as follows: "Contraction of the pupil to light is greatest when the light falls on the fovea centralis; stimulation of peripheral portions of the retina is followed by but slight contraction of the pupil." In close agreement with this statement is the claim made by Hess [1] that the pupillomotor fibres are concentrated into a small zone about the fovea, having a radius of 3 mm. All parts of this zone, he maintains, are uniformly sensitive, or approximately so. He states also that Heddaeus, more than twenty years earlier, had claimed that all the pupillomotor fibres are concentrated into a small area of the retina.

If the above statements are true, an increase in the area of the illuminated surface to which the pupil reacts beyond the projection of the pupillomotor zone should, it would seem, have very little effect on the size of the pupil, no more perhaps than might be expected as the result of the action of scattered light on the pupillomotor area. As will be shown later in the

[1] Hess, C., Untersuchungen über die Ausdehnung des pupillomotorisch wirksamen Bezirkes des Netzhaut und die pupillomotorischen Aufnahmeorgane, *Arch. f. Augenheilkunde*, 1907, **58**, 182–205; Investigations concerning the extent of the active pupillomotor area of the retina and concerning the pupillo-motor receiving organs, *Arch. of Ophthalmology*, 1909, **38**, 131–146; Das Differential-Pupilloskop, Eine Methode zur messenden Bestimmung von Störungden des Pupillenspieles, *Arch. f. Augenheilkunde*, 1916, **80**, 213–228.

paper, it not only has a great deal of effect, but the percentage effect for a given ratio of change in area is greater through that part of the retina which has the increased sensitivity to light, at least in dark adaptation, than it is through the more centrally located portions of the retina. In a later study the results of a more systematic investigation of the pupil's reaction to stimulation from center to periphery will be given.

Conditions under Which the Work was Done

The experiments were conducted in a darkened room. The observer was seated in front of a screen 150 cm square. The coefficient of reflection of this screen was 86 percent. The illuminated area which served as a stimulus for the reaction of the pupil was obtained by projecting the image of an evenly illuminated aperture on the screen.

Both the size and luminous intensity of these images were varied to satisfy the conditions of the experiment. The size was varied by varying the size of the aperture whose image was to be projected; the luminous intensity by varying the aperture of the lens used to project the image. The apparatus used was similar to one previously described by Ferree and Rand.[2] A photograph of this apparatus modified to suit our present purpose is given in Fig. 1. The apparatus is in effect a reconstructed Bausch and Lomb balopticon. A representation of the optical system of the apparatus is given in Fig. 2. The source of light is a 500-watt tungsten lamp of the type used in balopticons. This lamp is placed at the principal focus of a collimating system made up of two plano-convex lenses L_1, 11.5 cm in diameter. At a suitable distance in front of these lenses is a frame in which is mounted an iris diaphragm I_1, range of aperture 5×65 mm. This diaphragm is used to vary the size of the aperture which is to be imaged on the screen. Between the collimating system and the iris diaphragm is a thin plate of finely ground glass G to diffuse the light and to aid in providing the uniformity of illumination of the aperture that is needed. This diffusing plate is placed at a sufficient distance behind the iris diaphragm to eliminate all traces of mottling in the image which would occur were its illuminated surface imaged on the test field. Other features which ensure evenness of illumination of the aperture are (*a*) an etched concave mirror M, placed behind the source of light, which adds a component of diffused light to the beam; and (*b*) the size of the aperture which is made small enough in relation to the cross section of the collimating system L_1 to receive only the more central and homogeneous portion of the beam. The image is formed on the surface to be illuminated by a projection lens L_2 of suitable diopter value. Back of this lens and as close to it as is possible is mounted a second iris diaphragm I_2 provided with a pointer and calibrated scale. By varying the size of the aperture in this diaphragm the light density in the projected image is changed without changing the color of the light or the size or shape of the image. The size of the image is thus varied by the first diaphragm, and its luminous intensity by the second. Since this projection apparatus had to be placed at a slight angle with the receiving screen to avoid casting a shadow of the observer's head on the center of the illuminated field, the vertical dimension of the field was somewhat less than the hori-

[2] Ferree, C. E. and Rand, G., An apparatus for acuity, for mixing colored lights and testing light and color senses, *Jour. Exper. Psychol.*, 1927, **10**, 281–291; Intensity of illumination and other factors influencing the sensitivity of the radial line test for astigmatism, *Amer. Jour. Ophthal.*, 1929, **12**, 809–814.

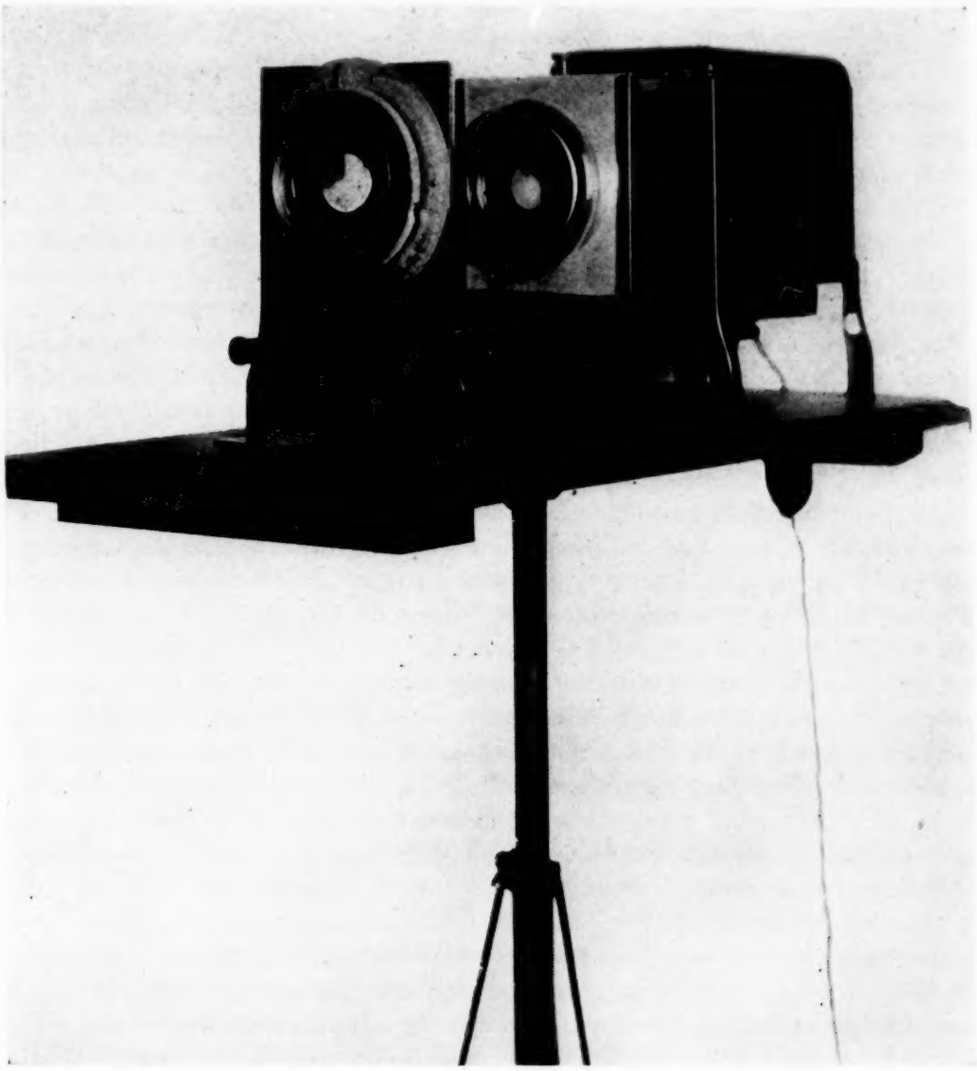


FIG. 1. Photograph of apparatus.

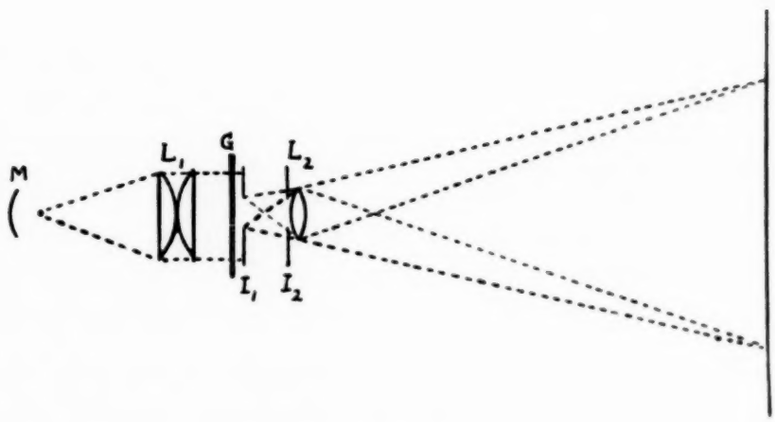


FIG. 2. Optical system.

zontal. Another result of the placement of the projection apparatus was a slight cosine effect on the evenness of illumination of the field.

The determination of the effect of size and intensity of illuminated field had to be made either in the presence of a third factor, convergence and accommodation, or a distance of 20 ft (6 meters) or greater had to be used. For the present series of experiments it was decided to use fixation distances which fell within the range of near work. Two distances were selected, 30 cm, which is near the accepted reading distance; and 50 cm, which is representative of a great deal of near work. For the smaller of these distances a wider range of visual angle and brightness of test field could be obtained than was possible for the greater distance. For the distance 30 cm illuminated fields subtending the following visual angles, horizontal diameter, were used: 20, 40, 60, 85, 100 and 115 degrees of arc. The brightness of each of these fields was in turn 1.16, 2.31, 4.63, 6.48, 9.25, 13.88 and 18.5 millilamberts. These brightnesses were obtained with values of incident light, respectively of 1.25, 2.5, 5, 7, 10, 15 and 20 ft.c. on the screen, the coefficient of reflection of which was, as stated above, 86 percent. For the distance 50 cm illuminated fields subtending the following visual angles, horizontal diameter, were used: 15, 30, 45, 60, 75, and 90 degrees. The brightness of each of these fields was in turn 1.16, 2.31, 4.63, 6.94, 9.25, 11.57 millilamberts. The values of the incident illumination giving these brightnesses were respectively 1.25, 2.5, 5, 7.5, 10 and 12.5 ft.c.

Constancy of position of the eyes under examination was secured by having the observer bite a mouth bit on which the impression of his teeth had previously been made and hardened in wax. The mouthbit was securely fastened to the vertical member of a mount which was adjustable in three dimensions. This mount was securely clamped to the table supporting the screen. To eliminate the effect of consensual reaction of the pupil due to difference in the stimulation of the two eyes, both eyes were kept open while the measurements were being made.

For the distance 30 cm, two observers, C and K, were employed. Observer C was an emmetrope aged 22 years, and K a myope aged 20 years. Observer K had the wide pupils which are so frequent in young myopes. Another reason for the wide pupils of this observer, as measured, was the fact that the measurements were made when her eyes were free from the influence of accommodation. That is, the screen was beyond her far point; there was thus no incentive for accommodation. With her accommodation relaxed blurred vision only of the fixation cross could be obtained. This was sufficient, however, to serve as a control for fixation. If she attempted to secure clearer vision of the cross by further relaxation of accommodation the pupils fluctuated so continuously as to render measurement very difficult. She was instructed, therefore, to make no effort to secure clear vision of the cross and to restrain as far as possible all reflex tendencies in this direction. Under these conditions the pupils became sufficiently stable in size to permit of measurement. Without the inhibitive influence of a fixed accommodation for near, the change of size of the pupil in reaction to changes both in intensity of light and area of illuminated field was greater for this observer than for the other observers. In short the results obtained may be regarded as somewhat representative of the order of change that may be expected for an eye free from the influence of accommodation. In this connection it must be remembered, however, that the eye under examination was myopic, not emmetropic.

For the distance 50 cm, one observer, R, aged 40 years, was used. This observer was also myopic, but did not have unusually wide pupils. Also the distance of the screen was well within the range of her accommodation. In case of both observers K and R, the measurements had of course to be made without correcting glasses.

Since the reaction of the pupil to light is dependent upon the sensitivity of the retina, a period of adaptation to the darkened room was allowed prior to each set of measurements. A period of dark adaptation was also allowed between each of the exposures to the illuminated fields. Further a period of exposure or adaptation to each of the illuminated fields had to be given sufficiently long to allow the pupil to come to a state of rest, or to attain sufficient stability of size to permit of accurate measurement. To secure reproducibility of results, great care had to be exercised in the control of these and other factors. Some difference was found in these requirements for the different observers.

The pupil was measured with the Ferree-Rand pupillometer. In order that neither this instrument nor the examiner should be within the field of view of the observer, the blind spot was mapped with the given fixation, and the end of the telescope of the instrument was inserted in a small hole in the screen cut within the blind area. The reactions of the observer were thus in no way disturbed by the presence and manipulation of auxiliary apparatus. When the light was projected on the test field the observer fixated a cross at the center of the field, with rest intervals, until the experimenter viewing the image of the pupil in the telescope was satisfied that stability of size had been reached. The measurement was then made by means of the calibrated scale in the image plane of the telescope. In order to prevent distortion due to the angle at which the eye was viewed, the vertical diameter of the pupil was measured. In case of all observers, there was not enough light reflected from the screen to the eye to permit of the measurement of the pupil for the lowest intensity of illumination and the smallest field size, and for one of the observers for the two lowest intensities and the two smallest field sizes. The reflecting power of the iris is of course an important factor in the amount of light that is needed for the measurement.

For a description and picture of the Ferree-Rand pupillometer, the instrument with which the measurements of the pupil were made, the reader is referred to a previous paper published in this *Journal*.[3] The features of the instrument may be roughly summarized as follows: It provides for the measurement of the breadth of the pupil to fractions of a millimeter within a small percentage of error; the method of making the measurement does not reduce the amount of light entering the eye or in any other way disturb the reaction to the condition being studied; the measurement may be continued without interruption through a considerable period of time; measurement is possible over a wide range of intensities; the instrument is easily and readily adaptable to a wide range of conditions of use.

Results

The results for the three observers are shown in Tables 1 to 5 and in Figs. 3 and 4.

In Tables 1 and 2 and Fig. 4 the effect on size of pupil fo change of brightness of the illuminated field is shown for the different sizes of field. The results for observers K and C are given in Table 1 and Charts *A* and *B* of Fig. 3. These results cover a brightness of field ranging from 1.16 to 18.5 millilamberts and a size of field ranging from 20 to 115 degrees of

[3] Ferree, C. E. and Rand, G., Relation of size of pupil to intensity of light and speed of vision and other studies, *Jour. of Exper. Psychol.*, 1932, **15**, 37–55.

TABLE 1

SHOWING THE EFFECT OF CHANGE IN BRIGHTNESS FOR DIFFERENT SIZES OF ILLUMINATED FIELD ON SIZE OF PUPIL (2 OBSERVERS)

Fixation distance 30 cm. Range of brightness of field, 1.16 to 18.50 millilamberts. Range of size of field, horizontal diameter, 20 to 115 degrees visual angle.

Visual Angle Subtended by Field (Degrees)	Brightness of Field (Millilamberts)	Value of Incident Illumination (Footcandles)	Diameter of Pupil, Observer K (Mm)	Change in Diameter of Pupil with Change of Intensity of Illumination		Diameter of Pupil, Observer C (Mm)	Change in Diameter of Pupil with Change of Intensity of Illumination	
				Mm	Percent*		Mm	Percent*
20	1.16	1.25	—			—		
	2.31	2.5	6.96			—		
	4.63	5.0	6.66			4.55		
	6.48	7.0	6.06			4.09		
	9.25	10.0	5.00			3.94		
	13.88	15.0	4.55			3.90		
	18.50	20.0	4.09			3.79		
40	1.16	1.25	6.96			4.77		
	2.31	2.5	6.90	0.06	0.9	4.62	0.15	3.1
	4.63	5.0	6.52	0.44	6.3	4.25	0.52	11.1
	6.48	7.0	5.76	1.20	17.4	3.86	0.91	19.1
	9.25	10.0	4.70	2.26	32.6	3.79	0.98	20.3
	13.88	15.0	4.40	2.56	36.8	3.72	1.05	22.0
	18.50	20.0	3.94	3.02	43.3	3.56	1.21	25.3
60	1.16	1.25	6.96			4.70		
	2.31	2.5	6.82	0.14	2.0	4.55	0.15	3.2
	4.63	5.0	6.36	0.60	8.6	4.09	0.61	13.0
	6.48	7.0	5.60	1.36	19.5	3.79	0.91	19.4
	9.25	10.0	4.55	2.41	34.6	3.64	1.06	22.6
	13.88	15.0	4.25	2.71	38.9	3.64	1.06	22.6
	18.50	20.0	3.78	3.18	45.7	3.48	1.22	26.0
85	1.16	1.25	6.82			4.55		
	2.31	2.5	6.66	0.16	2.3	4.47	0.12	2.6
	4.63	5.0	6.06	0.76	11.1	3.94	0.61	13.4
	6.48	7.0	5.30	1.52	22.3	3.64	0.91	20.0
	9.25	10.0	4.47	2.35	34.4	3.56	0.99	21.7
	13.88	15.0	4.09	2.73	40.0	3.48	1.07	23.6
	18.50	20.0	3.64	3.18	46.6	3.33	1.22	26.8
100	1.16	1.25	6.66			4.55		
	2.31	2.5	6.52	0.14	2.1	4.40	0.15	3.3
	4.63	5.0	5.76	0.90	13.5	3.86	0.69	15.2
	6.48	7.0	5.14	1.52	22.8	3.64	0.91	20.0
	9.25	10.0	4.40	2.26	33.9	3.48	1.07	23.6
	13.88	15.0	3.94	2.72	40.8	3.33	1.22	26.8
	18.50	20.0	3.64	3.02	45.4	3.33	1.22	26.8
115	1.16	1.25	6.52			4.40		
	2.31	2.50	6.36	0.16	2.5	4.25	0.15	3.4
	4.63	5.0	5.44	1.08	16.5	3.79	0.61	13.9
	6.48	7.0	5.00	1.52	23.3	3.56	0.84	19.1
	9.25	10.0	4.25	2.27	34.8	3.48	0.92	20.9
	13.88	15.0	3.79	2.73	41.8	3.33	1.07	24.4
	18.50	20.0	3.56	2.96	45.4	3.26	1.14	25.9

* In computing these differences the diameter of pupil at 1.16 ml for each size of field was taken as base.

TABLE 2

SHOWING THE EFFECT OF CHANGE IN BRIGHTNESS FOR DIFFERENT SIZES OF ILLUMINATED FIELD ON SIZE OF PUPIL (1 OBSERVER)

Fixation distance 50 cm. Range of brightness of field, 1.16 to 11.57 millilamberts. Range of size of field, horizontal diameter, 15 to 90 degrees visual angle.

Visual Angle Subtended by Field (Degrees)	Brightness of Field (Millilamberts)	Value of Incident Illumination (Foot-candles)	Diameter of Pupil, Observer R (Mm)	Change in Diameter of Pupil with Change of Intensity of Illumination	
				Mm	Percent*
15	1.16	1.25	—		
	2.31	2.5	4.50		
	4.63	5.0	4.25		
	6.94	7.5	4.05		
	9.25	10.0	3.75		
	11.57	12.5	3.62		
30	1.16	1.25	4.62		
	2.31	2.5	4.40	0.22	4.8
	4.63	5.0	4.12	0.50	10.8
	6.94	7.5	3.90	0.72	15.6
	9.25	10.0	3.60	1.02	22.1
	11.57	12.5	3.40	1.22	25.6
45	1.16	1.25	4.50		
	2.31	2.5	4.25	0.25	5.6
	4.63	5.0	4.00	0.50	10.8
	6.94	7.5	3.75	0.75	16.7
	9.25	10.0	3.55	0.95	20.9
	11.57	12.5	3.25	1.25	27.8
60	1.16	1.25	4.50		
	2.31	2.5	4.25	0.25	5.6
	4.63	5.0	3.90	0.60	13.3
	6.94	7.5	3.67	0.83	18.4
	9.25	10.0	3.50	1.00	22.2
	11.57	12.5	3.20	1.30	28.9
75	1.16	1.25	4.37		
	2.31	2.5	4.12	0.25	5.7
	4.63	5.0	3.87	0.50	11.4
	6.94	7.5	3.62	0.75	17.2
	9.25	10.0	3.40	0.95	22.0
	11.57	12.5	3.15	1.22	28.0
90	1.16	1.25	4.25		
	2.31	2.5	4.00	0.25	5.9
	4.63	5.0	3.75	0.50	11.7
	6.94	7.5	3.50	0.75	17.6
	9.25	10.0	3.30	0.95	22.3
	11.57	12.5	3.10	1.15	27.0

* In computing these differences the diameter of pupil at 1.16 ml for each size of field was taken as base.

TABLE 3

SHOWING THE EFFECT OF CHANGE IN SIZE OF FIELD FOR DIFFERENT BRIGHTNESSES OF ILLUMINATED FIELD ON SIZE OF PUPIL (2 OBSERVERS)

Fixation distance 30 cm. Range of size of field, horizontal diameter, 20 to 115 degrees, visual angle. Range of brightness of field, 1.16 to 18.5 millilamberts.

Brightness of Field (Millilamberts)	Value of Incident Illumination (Foot-candles)	Visual Angle Subtended by Field (Degrees)	Diameter of Pupil, Observer K (Mm)	Change in Diameter of Pupil with Change of Size of Field		Diameter of Pupil, Observer C (Mm)	Change in Diameter of Pupil with Change of Size of Field	
				Mm	Percent*		Mm	Percent*
1.16	1.25	20	—			—		
		40	6.96			4.77		
		60	6.96			4.70		
		85	6.82			4.55		
		100	6.66			4.55		
		115	6.52			4.40		
2.31	2.5	20	6.96			—		
		40	6.90	0.6	0.9	4.62		
		60	6.82	0.14	2.0	4.55		
		85	6.66	0.30	4.3	4.47		
		100	6.52	0.44	6.3	4.40		
		115	6.36	0.60	8.6	4.25		
4.63	5.0	20	6.66			4.55		
		40	6.52	0.14	2.1	4.25	0.30	6.6
		60	6.36	0.30	4.5	4.09	0.46	10.1
		85	6.06	0.60	9.0	3.94	0.61	13.4
		100	5.76	0.90	13.5	3.86	0.69	15.4
		115	5.44	1.22	18.3	3.79	0.76	16.7
6.48	7.0	20	6.06			4.09		
		40	5.76	0.30	4.9	3.86	0.23	5.6
		60	5.60	0.46	7.6	3.79	0.30	7.3
		85	5.30	0.76	12.5	3.64	0.45	11.0
		100	5.14	0.92	15.1	3.64	0.45	11.0
		115	5.00	1.06	17.5	3.56	0.53	13.0
9.25	10.0	20	5.00			3.94		
		40	4.70	0.30	6.0	3.79	0.15	3.8
		60	4.55	0.45	9.0	3.64	0.30	7.6
		85	4.47	0.53	10.6	3.56	0.38	9.7
		100	4.40	0.60	12.0	3.48	0.46	11.7
		115	4.25	0.75	15.0	3.48	0.46	11.7
13.88	15.0	20	4.55			3.90		
		40	4.40	0.15	3.3	3.72	0.18	4.6
		60	4.25	0.30	6.6	3.64	0.26	6.7
		85	4.09	0.46	10.1	3.48	0.42	10.8
		100	3.94	0.61	13.4	3.33	0.57	14.6
		115	3.79	0.76	16.7	3.33	0.57	14.6
18.50	20.0	20	4.09			3.79		
		40	3.94	0.15	3.7	3.56	0.23	6.1
		60	3.78	0.31	7.6	3.48	0.31	8.2
		85	3.64	0.45	11.0	3.33	0.46	12.1
		100	3.64	0.45	11.0	3.33	0.46	12.1
		115	3.56	0.53	13.0	3.26	0.53	14.1

* In computing these differences the diameter of pupil for the size of field subtending an angle of 20° for each brightness was taken as base.

TABLE 4

SHOWING THE EFFECT OF CHANGE IN SIZE OF FIELD FOR DIFFERENT BRIGHTNESSES OF ILLUMINATED FIELD ON SIZE OF PUPIL (1 OBSERVER)

Fixation distance 50 cm. Range of size of field, horizontal diameter, 15 to 90 degrees visual angle. Range of brightness of field, 1.16 to 11.57 millilamberts.

Brightness of Field (Millilamberts)	Value of Incident Illumination (Foot-candles)	Visual Angle Subtended by Field (Degrees)	Diameter of Pupil, Observer R (Mm)	Change in Diameter of Pupil with Change in Size of Field	
				Mm	Percent*
1.16	1.25	15	—		
		30	4.62		
		45	4.50		
		60	4.50		
		75	4.37		
		90	4.25		
2.31	2.5	15	4.50		
		30	4.40	0.10	2.2
		45	4.25	0.25	5.6
		60	4.25	0.25	5.6
		75	4.12	0.38	8.4
		90	4.00	0.50	11.1
4.63	5.0	15	4.25		
		30	4.12	0.13	3.1
		45	4.00	0.25	5.9
		60	3.90	0.35	8.2
		75	3.87	0.38	8.9
		90	3.75	0.50	11.8
6.94	7.5	15	4.05		
		30	3.90	0.15	3.7
		45	3.75	0.30	7.4
		60	3.67	0.38	9.4
		75	3.62	0.43	10.6
		90	3.50	0.55	13.5
9.25	10.0	15	3.75		
		30	3.60	0.15	4.0
		45	3.55	0.20	5.3
		60	3.50	0.25	6.7
		75	3.40	0.35	9.3
		90	3.30	0.45	12.0
11.57	12.5	15	3.62		
		30	3.40	0.12	3.3
		45	3.25	0.27	7.4
		60	3.20	0.42	11.6
		75	3.15	0.47	13.0
		90	3.10	0.52	14.4

* In computing these differences the diameter of pupil for the size of field subtending an angle of 15° for each brightness was taken as base.

TABLE 5

SHOWING THE EFFECT OF INCREASE OF SIZE OF ILLUMINATED FIELD ON SIZE OF PUPIL FOR EQUAL RATIOS OF CHANGE OF SIZE OF FIELD

The sizes are selected so as to involve in each case a comparison of the effect of the increase at different distances from the center of the field.

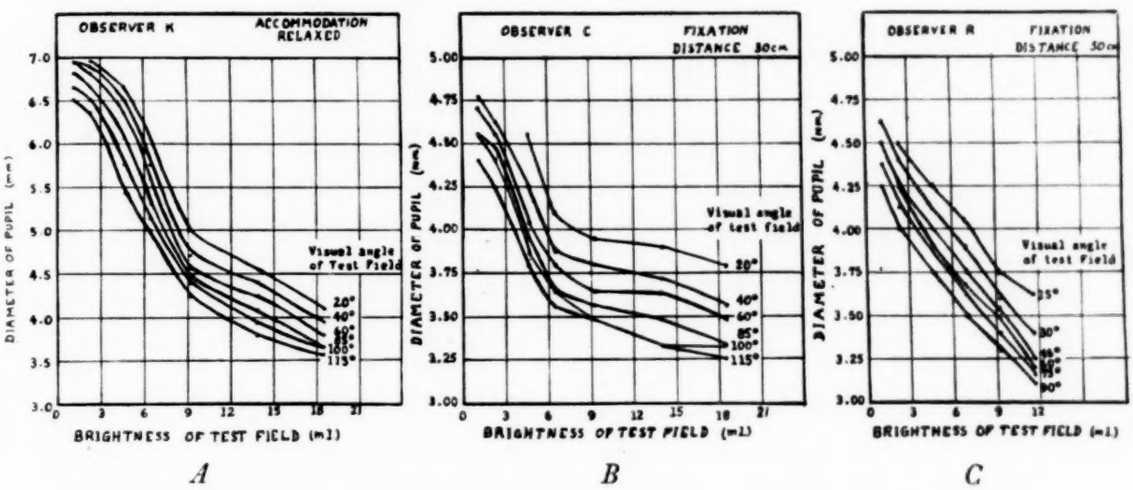
Observer	Change of Size of Illuminated Field: Horizontal Diameter in Degrees	Percentage Change in Size of Pupil, Value of Pupil for Smallest Field Being Taken as Bases of Computation: Brightness of Field (Ml) 2.31	4.63	6.48	9.25	13.9	18.5	Average
K	20°–40°	0.9	2.1	4.9	6.0	3.3	3.7	3.5
	30°–60°	1.7	3.8	5.7	6.6	5.3	6.2	4.9
	45°–90°	4.1	8.4	8.2	5.1	7.3	7.2	6.7
	55°–110°	6.3	14.0	10.6	6.5	10.3	6.8	9.1
C	20°–40°		6.6	5.6	3.8	5.6	6.1	5.5
	30°–60°		7.5	5.0	6.2	5.0	5.7	5.9
	45°–90°		7.1	5.5	6.1	7.3	5.9	6.4
	55°–110°		7.7	6.8	5.4	8.8	6.3	7.0
				(6.94 ml)		(11.57 ml)		
R	15°–30°	2.2	3.1	3.7	4.0	6.1		3.8
	30°–60°	3.4	5.2	5.9	2.8	5.9		4.6
	45°–90°	5.9	6.3	6.7	5.6	6.2		6.1

visual angle, horizontal diameter. The results for R are given in Table 2 and Chart *C* of Fig. 3. They cover a range of brightness of field from 1.16 to 11.57 millilamberts and of size of field from 15 to 90 degrees of visual angle, horizontal diameter. The charts in Fig. 3 represent for each size of field the variation of size of pupil with change of brightness of the field. Diameter of pupil in mm is plotted on the vertical coördinate [4] and brightness of field in millilamberts on the horizontal coördinate.

In Tables 3 and 4 and Fig. 4 the effect on size of pupil of change in size of the illuminated field is shown for different levels of brightness. The results obtained for observers K and C are shown in Table 3 and Charts *A* and *B* of Fig. 4. The sizes of field varied from 20 to 115 degrees of visual angle, horizontal diameter; the brightness of field from 1.16 to 18.5

[4] It will be noted that the ordinate values are twice as large for Chart *A* as for *B* and *C*. Observer K, it will be remembered, had the large pupils of the myope, and in addition was tested under relaxed accommodation.

millilamberts. The results for observer R are given in Table 4 and Chart *C* of Fig. 4. For this observer the size of field varied from 15 to 90 degrees of visual angle, horizontal diameter, and the brightness of field from 1.16 to 11.57 millilamberts. The charts in Fig. 4 represent for each brightness of field the variation in size of pupil with change in size of field. Diameter of pupil in mm [4] is plotted on the vertical coördinate and size of field in degrees of visual angle on the horizontal coördinate.



A *B* *C*

FIG. 3. Effect of change in brightness for different sizes of illuminated field on size of pupil (3 observers).

In Table 5 the effect of increase of size of illuminated field on size of pupil is shown for equal ratios of change of size of field. Diameters of field in the ratio of 1 : 2 were selected for comparison. Further, the sizes of field were selected so as to involve in each case a comparison of the effect of increase at different distances from the center of the field. The change of size of field for which the comparison is made is given in column 2 of Table 5. The corresponding percentage change in size of pupil is given in succeeding columns for each intensity of field. In computing these percentages the value of the pupil for the smallest field was taken as base. In the last column is given the average percentage change in size of pupil at the different intensities for the change in size of field under consideration. These data are given for each of the three observers.

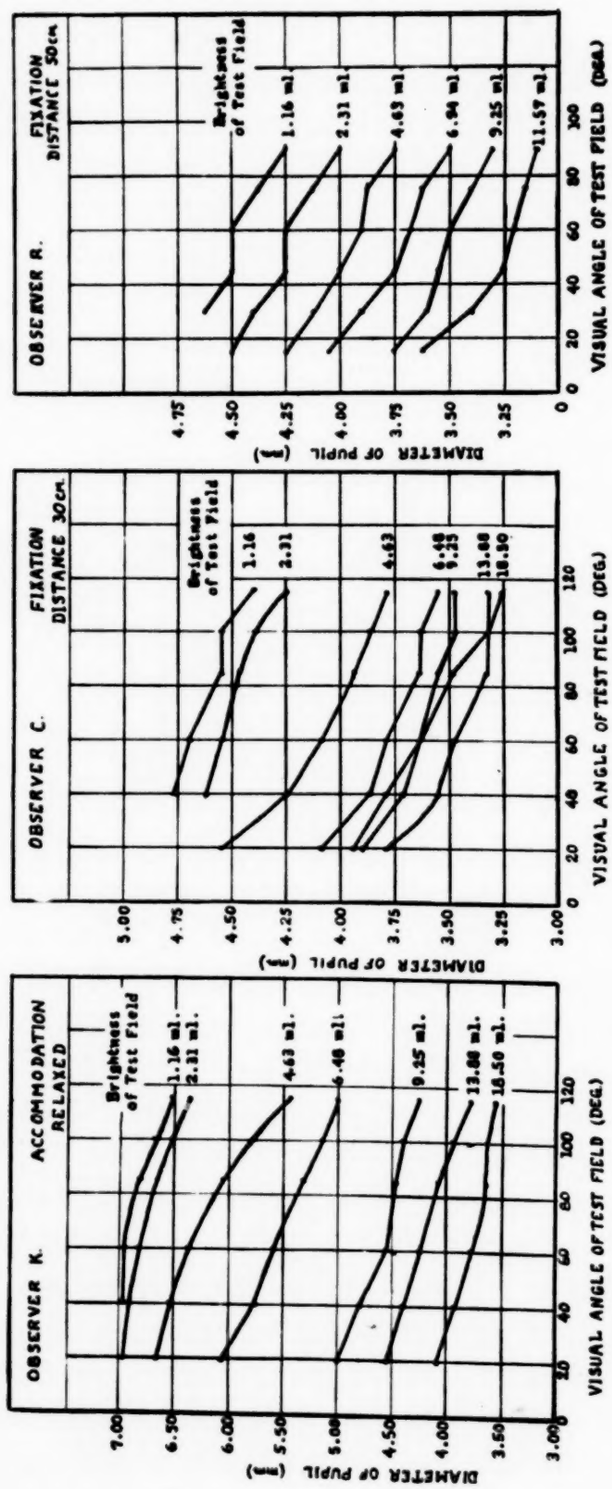


FIG. 4. Effect of change in size of field for different brightnesses of illuminated field on size of pupil (3 observers).

The results may be summarized as follows:

(1) Size as well as brightness of field affects the size of the pupil. In considering the effect of intensity of light on size of pupil we have thus always to take into account the size of the illuminated area as well as the intensity of its illumination. The effect, however. is by no means directly proportional to the increase in the area of the test object.

(2) Within the range of size and brightness of field used, change in the brightness of the field exerts a greater effect on the size of the pupil than change in the size of the field. The effect of change in size of the field thus does not seem to follow the same law as the effect of change of intensity.

(3) There is a fairly close correspondence in the effect of change of brightness of field on size of pupil for each of the sizes of field employed. Among the reasons which may be given for this conclusion the following may be noted. (*a*) The curves in Fig. 3 show a generally parallel course for each of the sizes of field used. And (*b*) as is shown in columns 6 and 9 of Table 1 and column 6 of Table 2, the percentage change in size of pupil for the different ranges of change of brightness from the minimum brightness employed (1.16 ml) is approximately the same for each size of field. This correspondence is closer throughout for observers C and R than for observer K. For the latter observer the percentages of change in size of pupil for the changes in brightness of field in the lower part of the scale are somewhat less for the smaller than the larger sizes of field.

(4) Similarly there is a correspondence in the effect of change in size of field on size of pupil for each brightness used, but in this case the correspondence is not so close. This can be seen from an inspection of the curves in Fig. 4 and of the percentages given in columns 6 and 9 of Table 3 and column 6 of Table 4. Again there is an exception for observer K. For this observer it will be noted that the percentages of change in size of pupil for changes of size of field are somewhat less for the lower than for the higher brightnesses of field.

(5) Increase of size of field not only has a great deal of effect on size of pupil, but the percentage effect for a given

ratio of change in size is greater through that part of the field for which there is an increased sensitivity to light, at least in dark adaptation, than it is for the more centrally located portions of the field. This would seem to be in direct contradiction to the statement quoted earlier in the paper that the stimulation of the peripheral portions of the retina is followed by but slight contractions of the pupil.

(6) The effect of change of size of field on size of pupil is not proportional to the total amount of light emitted or reflected from the field for the range of variations used in this experiment; *i.e.*, it is not proportional to the product of area of field and intensity of light.

(7) The knee of the curve for size of pupil plotted against brightness of stimulating field occurs for observer K at about 9.25 ml (10 ft.c.) and for observer C at 6.45 ml (7.5 ft.c.). For observer R it was not reached at the highest brightness of field used, 11.57 ml (12.5 ft.c.). It was shown in a former paper[5] that the knee of the curve showing the relation between size of pupil and the illumination of the stimulating field occurred for the six observers used between 10 and 20 ft.c. The conditions of work in that study were very different, however, from those of the present study. They comprised a fairly uniform illumination of the entire field of view of the observer, and a fixation distance of 2.5 meters.

(8) There is no pronounced knee in the curve showing size of pupil plotted against size of field for the range of variations used in this study.

(Manuscript received March 15, 1932)

[5] Ferree, C. E. and Rand, G., Relation of size of pupil to intensity of light and speed of vision, and other studies, *Jour. Exper. Psychol.*, 1932, 15, 37–55.

RUNWAY TIME AND THE GOAL GRADIENT

BY AMOS C. ANDERSON
Ohio University

Problem and Method

In an experiment reported elsewhere the writer [1] obtained incidentally some data which it seems worth while to present in view of Hull's [2] recent article on the goal gradient hypothesis.

The problem being investigated was that of time discrimination in the white rat. The apparatus used in the part of the problem which concerns us here was of the multiple choice type, consisting of a runway 7 feet in length leading to 2 detention chambers. The animal, after being released from the starting box, entered one of the detention chambers in which he was confined for a given length of time, after which he was allowed to go on into the food box. His problem was to learn to enter the chamber in which he was confined for the shortest time. For example, each animal of one group would be confined 1 minute if he entered one of the chambers, but 2 minutes if he entered the other one. In either case he was allowed a constant time (20 seconds) for eating after the period of detention. Incidentally to this experiment, the time spent in the runway, *i.e.* the time elapsing between the release of the animal and his entering a detention chamber, was recorded. It is this phase of the experiment which is here reported. These data are not available for all the animals used in the experiment, but are given for the groups for which they are available. As each animal made 5 runs per day for from 40 to 100 days, it will be seen that the original data tables are too extensive to present here. The procedure followed was to compute the average number of seconds per

[1] Anderson, Amos C., Time discrimination in the white rat, *Jr. Comp. Psychol.*, 1932, XIII, No. 1, 27–55.

[2] Hull, Clark L., The goal gradient hypothesis and maze learning, *Psychol. Rev.*, 1932, **39**, 26–43.

TABLE I

Ratio	Detention Times	No. of Animals in Group	Average Number of Seconds per Run per Rat for Successive 5 Day Intervals (25 Runs)																			
			1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
1 : 3	2 and 6 min.	4	56	48	40	38	33	22	21	17												
	1 and 3 min.	8	27	17	15	13	10	9	9	7	6	6										
	30 and 90 sec.	4	50	18	14	13	13	13	11	9	9	9										
	10 and 30 sec.	4	44	15	16	9	9	10	10	10	9	11										
1 : 2	1 and 2 min.	12	52	25	21	18	12	11	12	10	11	11										
	30 and 60 sec.	8	37	20	17	16	11	11	10	10	10	11										
	10 and 20 sec.	4	15	12	11	13	7	8	8	7	11	8										
1 : 1.5	2 and 3 min.	4	29	24	23	20	15	18	21	24	22	22	26	26	24	22	26	26	25	19	29	30
	1 and 1.5 min.	4	30	20	18	18	13	13	12	13	12	11	13	9	9	18	19	17	25	19	13	12

run per rat for successive 5-day intervals. This material, presented in graphical as well as tabular form, follows.

Results

The first set of graphs (Fig. 1) shows the time spent on the runway by different groups of rats having different absolute detention times, all in the ratio 1 : 3. These groups learned to

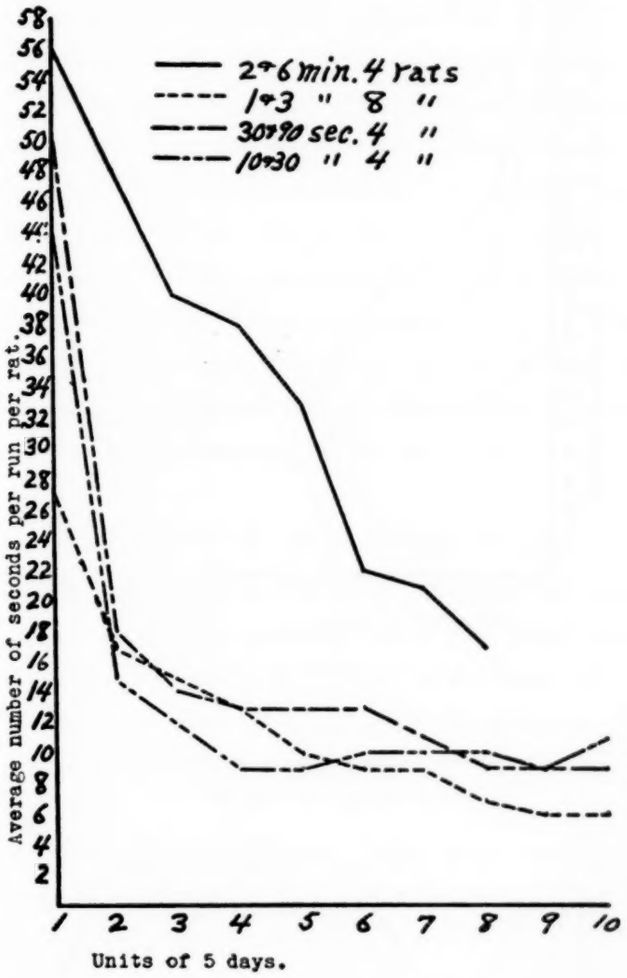


Fig. 1.

make the discrimination about equally well, as shown elsewhere, the relative differences rather than the absolute differences determining the speed of learning. But it will be seen that the group having the 2 and 6 minute detention times dallied on the runway much more than the other groups which were not detained so long on the way to the food. The 30 and 90 second group, for most of the 5 day periods into which the

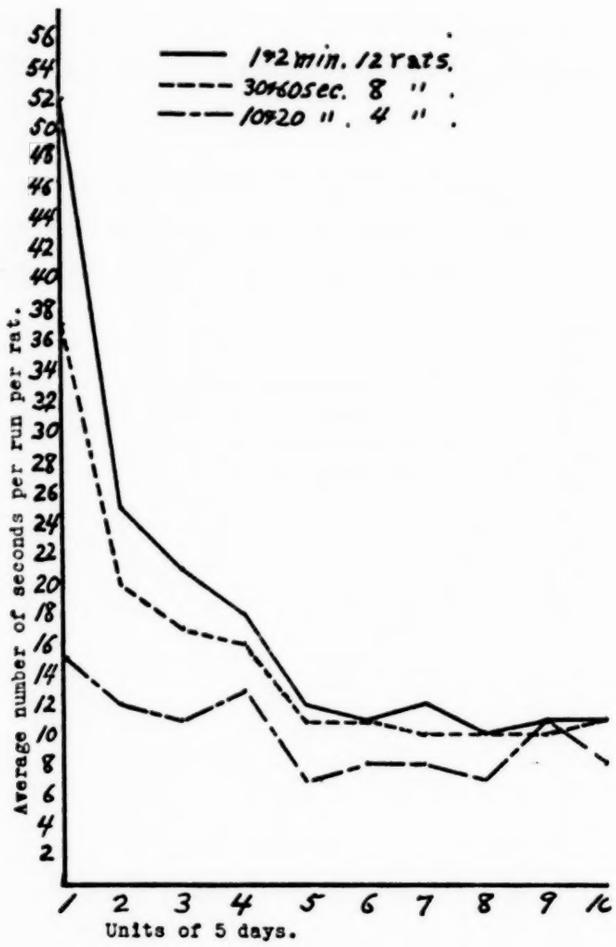

1-2 min. 12 rats.
30-60 sec. 8 " .
10-20 " . 4 " .
Average number of seconds per run per rat.
Units of 5 days.

FIG. 2.

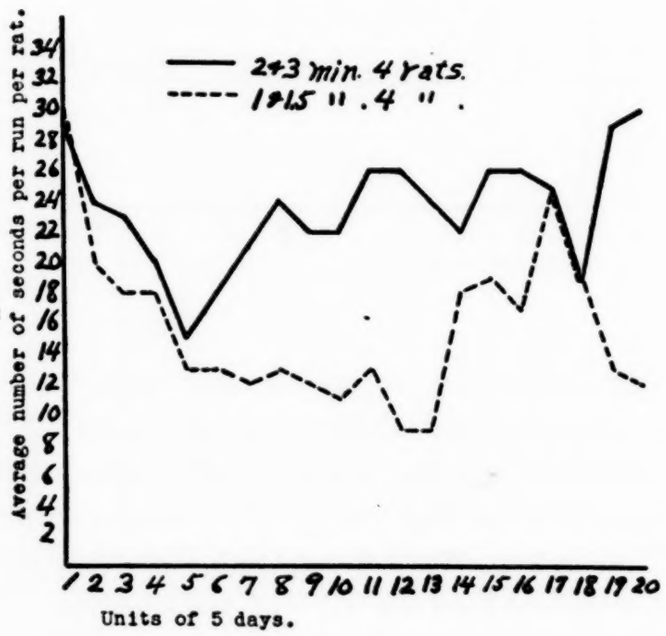

2-3 min. 4 rats.
1-1.5 " . 4 " .
Average number of seconds per run per rat.
Units of 5 days.

FIG. 3.

data are divided, spent more time on the runway than the 10 and 30 second group. The 1 and 3 minute group constitutes an exception for which the writer can offer no explanation. It might be pointed out, however, that of the 9 groups of animals for which data are presented this group is the only one not conforming in some measure to the tendency here indicated.

Similarly Fig. 2 shows the time spent on the runway by 3 groups of animals, all having detention times in the ratio 1 : 2, but differing for different groups in absolute time. These 3 groups likewise learned about equally quickly, but less quickly than those whose detention times were in the ratio 1 : 3. But in spite of their practical equality in learning to make the discrimination, they show the same tendency to dally on the runway more in proportion to the actual (absolute) amount of detention on the way to the food. The same may be said for Fig. 3, showing the time spent on the runway for 2 groups, one having 2 and 3 minute detention times, the other 1 and 1.5 minutes, both being in the ratio 1 : 1.5.

Discussion and Conclusion

Hull states the goal gradient hypothesis as follows: ". . . that the goal reaction gets conditioned the most strongly to the stimuli preceding it, and the other reactions of the behavior sequence get conditioned to their stimuli progressively weaker as they are more remote (in time or space) from the goal reaction."

We have in the experiment here reported a situation in which the animals having the longer absolute times, as *e.g.* those having detention times of 3 and 6 minutes, are farther removed from the goal (in time) than those having the shorter times, as 10 and 20 seconds. According to the hypothesis that part of the behavior sequence which consists in running from the starting box into the detention chamber, being more remote from the goal reaction in the case of the former group, gets conditioned less strongly to it than in the case of the latter group (having shorter detention times). As a consequence of this, the group having the longer detention times traverse the runway less rapidly, or dally more either in

starting or in entering one of the detention compartments. Indeed, Hull practically predicts this result when he says in one of his deductions from the principle: "That animals in traversing a maze will move at a progressively more rapid pace as the goal is approached." Those animals having the shorter confinement times are temporally nearer to the goal at the start, and may be expected to run off the series of acts preceding the final goal reaction more rapidly than those having longer detention periods and hence being temporally farther removed from the goal. This, barring extraneous factors, follows from the deduction quoted, and would be the most reasonable expectation. In this experiment the delay was mainly at the start and in making the entrance into the detention compartment. The animals in all groups soon came to move mainly at a run, so that any differences in the speed of locomotion were slight and could have been determined only by automatic recording devices. If the admittedly somewhat sketchy data presented here represent the true state of affairs we might state as a corollary to Hull's eighth deduction, quoted above, that animals traversing a maze or other pathway leading to a goal will perform the series of acts preceding the final goal reaction more rapidly the shorter (temporally or spatially) the pathway.

The above data are presented, then as confirmatory to the goal gradient hypothesis in that the tendency indicated here conforms to what would have been the most reasonable *a priori* prediction from the hypothesis and hence is deducible from it.

(Manuscript received February 27, 1932)

THE RELATIVE RETENTION VALUES OF STYLUS AND MENTAL MAZE HABITS[1]

BY R. H. WATERS AND GRACE B. POOLE
University of Arkansas

That the current generalization to the effect that motor habits are better retained than verbal habits has insufficient experimental support has been indicated by McGeoch and Melton in a recent paper.[2] Their study shows that, when the two types of material are learned to an arbitrarily equal degree, the comparative retention is a function of the particular comparison made. This fact suggests the hypothesis that the seeming better retention of motor habits may be due, in part, to the fact that they have been over-learned to a greater extent than have verbal materials. The present experiment was designed to test this hypothesis.

In selecting the materials for the experiment the effort was made to select those that appeared to be as nearly similar as possible and at the same time to represent what is generally accepted as being a motor habit on the one hand and a verbal habit on the other. The Warden maze, pattern I,[3] was selected for the one, and the Peterson mental maze[4] chosen for the other. The Warden maze presents a series of ten choices and the Peterson maze was so constructed as to present ten choices. In both mazes retracing was prohibited. In the stylus maze this was effected by using a block which just filled the short connecting pathway between any two choices. In the mental maze, when the subject made an incorrect choice, the experimenter called the same two alternatives again.

Three groups, of 20 university students each, served as

[1] Research Paper No. 277, Journal Series, University of Arkansas.

[2] J. A. McGeoch and A. W. Melton, The comparative retention values of maze habits and of nonsense syllables, *J. Exper. Psychol.*, 1929, **12**, 392–414.

[3] C. J. Warden, Primacy and recency as factors in cul-de-sac elimination in a stylus maze, *J. Exper. Psychol.*, 1924, **7**, 98–116.

[4] Joseph Peterson, Learning when frequency and recency factors are negative, *J. Exper. Psychol.*, 1922, **5**, 270–300.

subjects. Of these one group learned both the mental and motor habits to one perfect run; a second learned the motor habit to one perfect run and the mental habit to three perfect and then took ten more runs; the third learned the mental to one perfect repetition and the motor habit to three perfect runs and then took ten more runs. For all groups the order of learning was counter-balanced. Ten subjects of each group learned the motor habit first, returned after a week for a retention test and then immediately learned the mental maze habit, returning after the lapse of another week for a retention test on it. The remaining ten subjects of each group reversed this order.

In all retention tests the subjects were required to relearn both habits to a criterion of three perfect successive runs. Records of time, errors, and trials were kept and the comparative retention values were determined in terms of the saving scores of the two types of material.

The ordinary maze technique was employed. A screen between the subject and experimenter made it impossible for the subject to get any clues from the experimenter. The usual verbal instructions introduced the subjects to the nature of the problem. At the beginning of each retention period the subjects were questioned concerning review. None reported more than an occasional revival of the preceding setting and there was no evidence that this was more prevalent for the one habit than for the other.

The experimental results are summarized in the following tables. In view of the fact that the distributions were somewhat skewed and that the medians give the more stable values all comparisons were made in terms of medians.[5] It may be added, however, that the general tendency of the results was not disturbed by the use of the median values. Table 1 gives the learning and relearning data for both the stylus and mental mazes when learned to one perfect repetition (Condition I), and relearned to a criterion of one and three perfect runs. The learning and relearning data for the condition in which the stylus maze was learned to one perfect run

[5] Robert Leeper, The reliability and validity of maze experiments with white rats, *Genet. Psychol. Monog.*, 1932, 41, 137–245.

and the mental maze to a criterion of three perfect and ten more repetitions (Condition II), are given in table 2. Table 3 contains the data for the condition in which the motor maze was similarly over-learned and the mental maze learned to one

TABLE 1

MEDIAN SCORES. CONDITION I

	Learning		Relearning			
			Stylus		Mental	
	Stylus	Mental	1	3	1	3
Trials	24	5	6	14	1	2
Errors	84	15	14	27	3	4
Time (sec)	1033	359	181	341	56	87

TABLE 2

MEDIAN SCORES. CONDITION II

	Learning		Relearning			
			Stylus		Mental	
	Stylus	Mental	1	3	1	3
Trials	26.5	12.0	6.0	12.5	1.0	1.0
Errors	91.0	43.0	14.0	20.5	1.0	1.0
Time (sec)	1166.5	613.0	185.0	367.0	34.0	36.5

TABLE 3

MEDIAN SCORES. CONDITION III

	Learning		Relearning			
			Stylus		Mental	
	Stylus	Mental	1	3	1	3
Trials	34.5	6.0	1.0	1.0	3.0	3.0
Errors	116.5	26.5	1.0	1.0	4.5	5.0
Time (sec)	1278.0	338.5	23.5	30.5	100.0	120.0

perfect run only (Condition III). All records are exclusive of the criterial runs.

It should be expected that the learning and relearning records for the stylus maze under conditions I and II and those

for the mental maze under conditions I and III would be about the same. There is some difference and in table 4 these differences and their sigmas are tabulated. From this table it can be seen that all differences are highly unreliable save that between the trials for relearning the mental maze. The fact that the differences in all other cases are unreliable gives us confidence in believing that consistent results would be found with other samples.

TABLE 4

DIFFERENCES BETWEEN LEARNING AND RELEARNING FOR STYLUS MAZE, CONDITIONS I AND II, AND FOR MENTAL MAZE, CONDITIONS I AND III

	Stylus Maze				Mental Maze			
	Learning		Relearning		Learning		Relearning	
	D	Sigma	D	Sigma	D	Sigma	D	Sigma
Trials.....	2.5	6.67	0	2.68	1.0	1.88	2.0	.63
Errors.....	7.0	21.67	0	6.84	11.5	7.18	1.5	1.96
Time.....	133.5	441.92	4	69.49	20.5	79.20	44.0	26.13

As can be observed from tables 1 to 3, the mental maze, although appearing logically to be equal in degree of difficulty to the stylus maze, was far more readily learned than the stylus maze. Probably this difference in degree of difficulty is in part due to the fact that in learning the stylus maze the subject is compelled to learn the whole maze as a spatial unit within which the various choices come, while no such spatial unity characterizes the mental maze so far as the subject's learning is concerned. Whatever the cause for the greater degree of difficulty of the stylus maze, the comparison of the relative retention of the two habits is made the more interesting due to the fact that we can also determine whether the relative ease of the two problems compared has any appreciable influence on the relative degree of retention. It might be argued, for example, that the greater retention of motor habits is due to the greater degree of difficulty experienced in their mastery. That the degree of difficulty is not influential in determining relative retention is shown by the saving scores given in table 5. These saving scores are computed on the

TABLE 5

SAVING SCORES

Condition		Both Relearned to One Perfect Run		Both Relearned to Three Perfect Runs	
		Stylus	Mental	Stylus	Mental
I	Trials	79.45	70.83	47.16	59.82
	Errors	86.79	86.18	71.74	81.27
	Time	85.08	84.03	69.47	71.16
II	Trials	75.56	93.54	60.03	93.33
	Errors	85.46	98.99	77.26	98.56
	Time	85.45	95.26	74.00	94.57
III	Trials	96.10	66.66	94.58	63.33
	Errors	98.70	82.96	97.52	81.30
	Time	98.96	74.95	96.61	73.05

basis of the saving score of each individual and the table contains the median individual saving scores for the three experimental conditions.

The interpretation of the data contained in table 5 is apparently obvious. When both are learned to the same degree both are retained approximately equally well. The degree of retention varies with the method of measuring learning, *i.e.*, with but one exception in all the thirty-six measures the greatest savings are found in terms of errors, with time next, and with trials showing least saving. When the degree of required relearning is increased all saving scores are decreased. When the mental maze is over-learned it is retained significantly better than the mental maze habit barely learned. This situation holds in spite of the fact that the mental maze is much more readily learned. On the basis of the data from this experiment, then, we may conclude that the relative retention of motor and verbal habits is determined by the relative degree of learning of the two.

In addition to the data from the saving scores, a count of the number of subjects excelling in the degree of saving of the one or the other maze for each condition supports the conclusion stated above.

At the close of the experimental work each subject was asked to describe the method he employed in learning. The

question was asked in the form of Warden's questionnaire, *i.e.*, they were asked whether they learned each maze in verbal, visual, or motor terms. As a result of this inquiry it was found that the majority of the subjects had utilized some form of a verbal method in learning the stylus maze as well as the mental maze habit. This finding seems to raise a question regarding the interpretation of the whole experiment, as well as that of other experiments that have been reported on this problem. We thought we had selected one habit from the motor field and another from the verbal field. As a matter of fact it seems that what we do have are two habits which differ only in the relative amounts of verbal and motor components. When this fact was disclosed, that both were learned at least partly in verbal terms and hence were not two distinct types of habit, we cast about for some other form of learning that would get away from this difficulty but so far have been unsuccessful in discovering any means for circumventing it. If there are ways of avoiding this difficulty the problem of the relative retention values of motor and verbal habits is still open, since, theoretically at least, a motor habit is relatively free from any form of ideational or verbal learning and vice versa.

We have said 'theoretically at least,' but this might be questioned. Are there distinct types of learning as has been assumed? Or are there simply different aspects of learning which manifest themselves differently in different learning tasks? Or is not learning an extremely complicated process which, in any situation, goes on in a number of different, but coördinate, ways? Some data to be published in a later paper indicate that the latter is true at least for stylus maze learning.

In view of these considerations the conclusion to be drawn from this experiment should be couched as follows: The relative retention of what has been conventionally called 'motor' and 'verbal' habits is a function of the relative degree of learning of the two habits compared.

(Manuscript received March 14, 1932)

THE RELATION BETWEEN TWO METHODS OF LEARNING PIANO MUSIC[1]

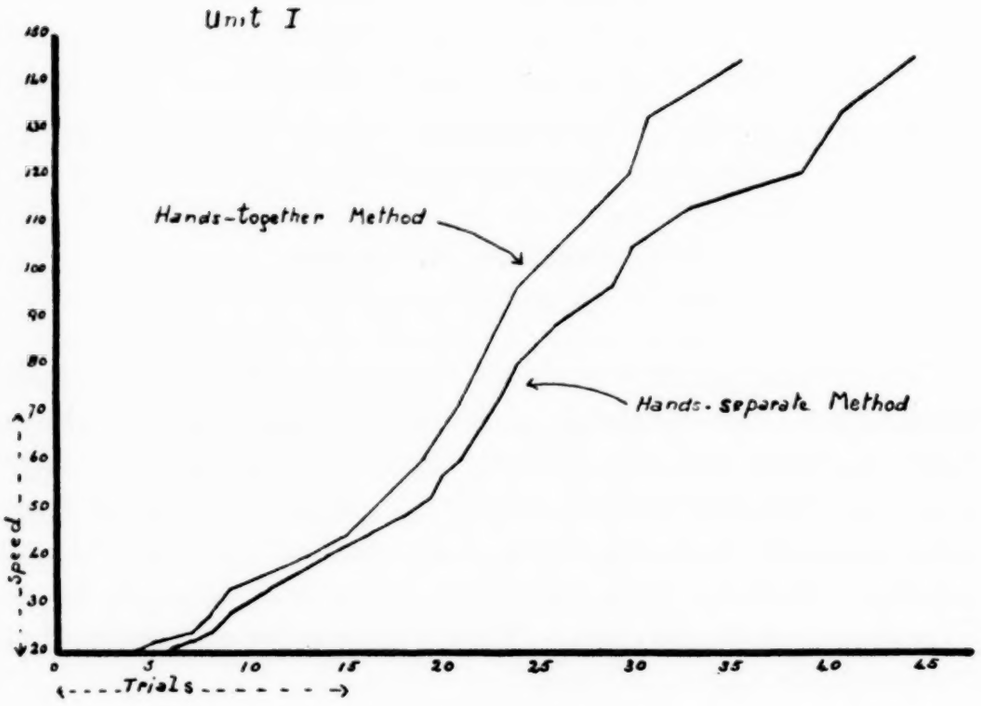
BY ROBERTA W. BROWN

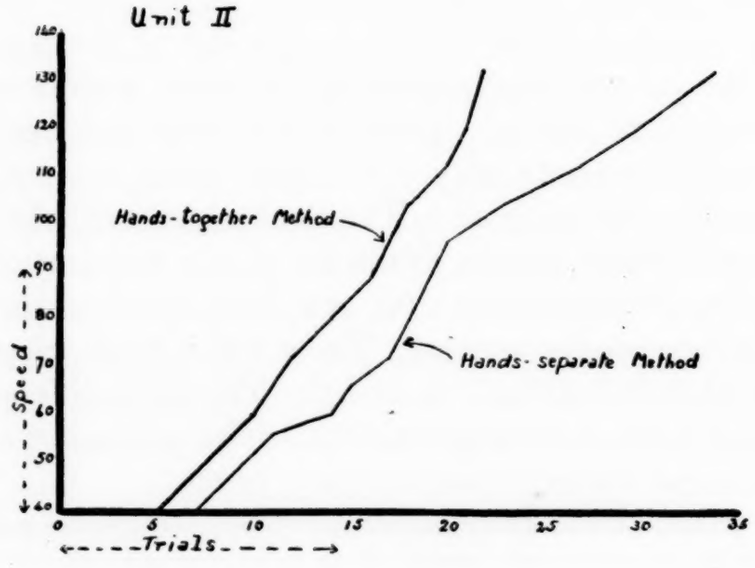
University of Chicago

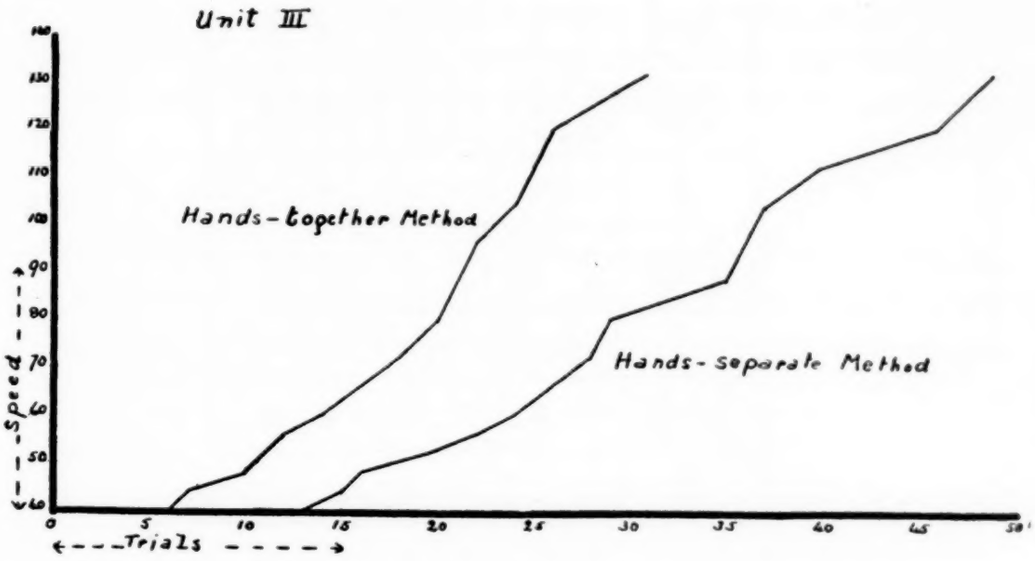
The object of this problem was to ascertain the relative efficiency of two methods of learning piano music. One of these methods was the 'Hands-separate' method; the other was the 'Hands-together' method. Pairs of musical scores were equated and one score was learned by the 'Hands-together' method, and the other score was learned by the 'Hands-separate' method. Then the relative efficiency of the two methods was computed.

The scores selected were those commonly given to music pupils and they were suitable to the ability of the learner, the writer. None of them had ever been seen by the writer. These scores were equated as to the number of measures, speed objective, rhythm and approximate number of notes. In order to assure, still farther, the equality of the pairs to be chosen the scores were presented to music pupils who had never seen them before. These pupils were required to read them at sight and to play accurately using what time was necessary for that purpose. The final selection of the pairs to be used was based on the similarity of the summation of the pupils' time requirements plus the time required for similar sight reading by the writer. Three pairs were selected and they were labeled, units 1, 2, and 3. The pairs differed, 9 sec, 14 sec, and 2 min 2 sec respectively. The last unit was longer and contained almost twice as many notes.

[1] To Dr. Thelma Gwinn Thurstone, without whose encouragement and guidance, this study might never have been completed, the writer expresses gratitude. The first problem of the series, Efficiency in Piano Learning, appeared in the *J. of Ex. Psych.*, June 1928.


Unit I
Hands-together Method
Hands-separate Method
Speed
Trials
0
5
10
15
20
25
30
35
40
45


Unit II
Hands-together Method
Hands-separate Method
Speed
Trials
0
5
10
15
20
25
30
35



'Constants' in Learning Process

Method

The scores were played by the 'whole' method, that is, from beginning to end without stopping. At no time were repetitions of difficult measures made in any of the learning.

Repetitions

Three repetitions of each score were made at a sitting. The score learned by the 'Hands-together' method was repeated in that way three times. Then the test was taken. The score learned by the 'Hands-separate' method was learned, first by playing the notes on the treble clef with the right hand, second, by playing the notes on the bass clef with the left hand and, third, by playing both clefs together with both hands. This was followed by the test.

Metronome

During all the learning and the tests the metronome was used. This promised to ensure equality of time consumed in learning each score. However, it was found that, in actual practice, variations were inevitable especially at the beginning of learning where the difficulties seemed more numerous.

Speed

The speed increased in regular progression throughout the learning. In two of the units, learning was begun at 40 beats

TABLE I

DATA ON SCORES

Unit	Name of Score	Composer	Page	Number	Number of Measures	Key	Speed Objective /M	Time	Total Pupils' Time	Number of Notes
I	(*a*) Minuet (First Year Classics) Foote	Bach	11		24	C Min	144	¾	6 min 14 sec	151
	(*b*) Stately Dance (First Year Classics) Foote	Bach	26		24	F Maj	144	¾	6 min 5 sec	197
II	(*a*) Sonatina (Op. 55, No. 1) (Schirmer's Library) Vol. 51	Kuhlau	22	4	26	C Maj	132	¼	6 min 24 sec	325
	(*b*) Sonatina (Op. 36, No. 5)	Clementi	51	5	26	G Maj	132	¼	8 min 22 sec	411
III	(*a*) Rondo (First Year Classics) Foote	Bach	8		24	C Min	132	¾	6 min 55 sec	204
	(*b*) Menuetto II (Schirmer's Library) Vol. 381	Händel	6	3	24	F Maj	132	¾	6 min 41 sec	196

to the minute and in one unit, learning was begun at 20 beats to the minute. As soon as a correct repetition was rendered during a test the metronome was moved down to the next speed.

Test

In determining correctness, all artistic values were ignored, *e.g.* accent or expression marks etc. Stoccato marks were recognized, however, and their neglect was regarded as an error.

Rotation

The scores were learned once a day at the same time, and the order of learning was alternated each day so that neither score would have the benefit of freshness of attack or warming up. The scores were also rotated in the three units in regard to difficulty. In two units the 'Hands-together' method was assigned to the more difficult scores and in one unit it was allotted to the easier score.

TABLE II

RESULTS

Unit	Score	Method	Total Time of Pupils' Performance (Min)	Order of Difficulty	Total Practice Time Required to Learn (Min)	Total No. Trials Required to Learn	Order of Completion
I	Minuet—11....	Together	6.23	2	163.45	36	1
	Dance—26.....	Separate	6.08	1	202.93	45	2
II	Sonatina—22...	Together	6.4	1	109.43	22	1
	Sonatina—51...	Separate	8.37	2	156.33	34	2
III	Rondo—8.....	Together	6.92	2	109.68	31	1
	Menuetto—6...	Separate	6.68	1	172.82	49	2

USE AND LIMITATIONS OF THE METRONOME

The slowest speed actually registered by the metronome was M♩ = 40 i.e. 40 beats to the minute. If a score written in $\frac{3}{4}$ time was played at the speed of M♩ = 40 each measure required $3 \times .025$ min or .75 min, and therefore a score containing 24 measures required 1.8 min, and for three

repetitions 5.4 minutes. In actual practice the average practice time for three repetitions of 24 measures of $\frac{3}{4}$ time played at M♩ = 40 was 5.63 minutes.

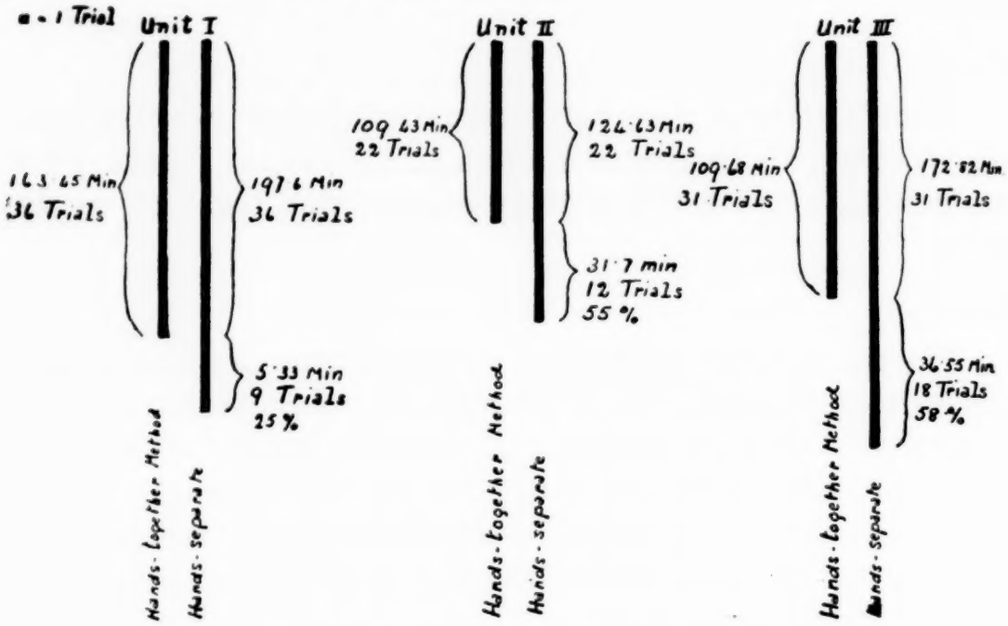


FIG. 4. A Graphic Representation of Data Given in Table IV.

Again, in playing 26 measures (unit III) at 4/4 time three times over required 3 × 26 × 4 × .025 min or 7.8 minutes. The average time required was 7.63 minutes.

The gradations on the metronome were not uniform so

TABLE IV

A STATISTICAL COMPARISON BETWEEN THE TWO METHODS

Unit	Hands Together Method		Hands Separate Method			Differences for Equal No. of Trials		Additional Requirement for Equal Speed Achievement		
	Summation of Min. Requirement	Total Trials	Total Time Requirement for Equal Number of Trials	Total Time for Equal Speed Achievement	Total Trials for Equal Speed Achievement	Minutes	Percent of Minutes	Minutes	Trials	Percent of Trials
	A	B	C	D	E	C–A		D–C	E–B	
I	163.45	36	197.6	202.93	45	34.15	21%	5.33	9	25%
II	109.68	31	136.27	172.82	49	26.59	24%	36.55	18	58%
III	109.43	22	124.63	156.33	34	15.2	14%	31.7	12	55%

that increases in speed could not be maintained in an entirely regular manner throughout the whole learning. As measured by the metronome, speed was increased by 2 degrees from a rate of ♩ = 20 to ♩ = 24; then from ♩ = 28 to ♩ = 33 and from there to 40 when it was increased by fours to ♩ = 60. It was increased by sixes from ♩ 60 to ♩ = 72; by eight degrees from ♩ = 72 to 120, and by gradations of twelve from ♩ = 120 to ♩ = 144.

Conclusions

1. The 'Hands-together' method was more efficient in both speed and trials than the 'Hands-separate' method.

2. The 'Hands-separate' method was progressively inefficient owing to the fact that the music for each hand became partially memorized which militated against their combination.

3. Pleasure in learning was more in evidence in the 'Hands-together' method.

(Manuscript received February 28, 1932)

LEARNING AS A FUNCTION OF WATER TEMPERATURE [1]

BY EUGENE ROLLAND HACK

The effect of temperature as an incentive to swimming activity in the rat has been considered and summarized by Wever.[2] In his investigation it was shown that the performance of rats previously trained in a simple water tank was a function of temperature, for the rate of swimming changed greatly as the water temperature was varied from 10° to 45° C. The present investigation was arranged to study the process of learning a simple water maze as a function of the different incentives imposed by distinct water temperatures.

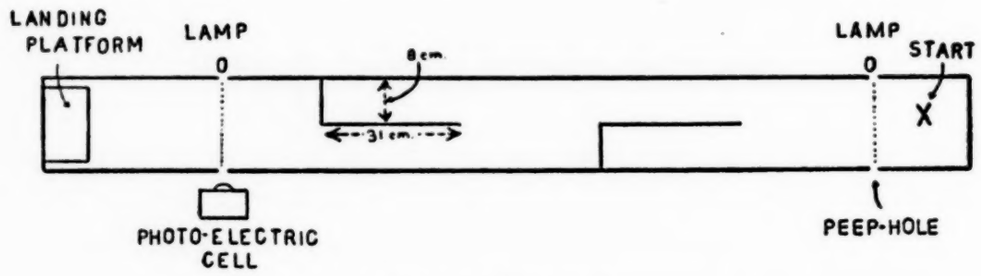


FIG. 1. Water tank.

The tank, as shown in Fig. 1, consisted of a straight, non-scalable metal trough 460 cm. long and 16 cm. wide and filled with water to the depth of 24 cm. Two blind alleys were inserted, as shown in the figure, at distances of 66 cm. and 217 cm. from the starting point. The rats were dropped in at one end and taken out at the other after they had swum to the landing platform. Time scores for each animal's performance in traversing the length of the trough were obtained by means of a relay-controlled stop watch. The watch was started by the experimenter who pressed a key when the rat first became visible in swimming past a peep-hole 115 cm. from the starting

[1] Minor Study from the Princeton Psychological Laboratory.

[2] E. G. Wever, Water Temperature as an Incentive to Swimming Activity in the Rat. *Jour. Comp. Psychol.*, 1932, **14**, 219–224.

point; it was stopped by the animal interrupting a light beam in a photo-electric circuit as it swam past a point 60 cm. from the landing platform. The distance over which the rats were clocked was 285 cm.

Three water temperatures were used, 15°, 37.5° and 45° C. By repeated checking during the trials these temperatures were maintained within a 1° variation.

Twenty-four albino rats, 12 males and 12 females from the laboratory colony, were used. Their ages ranged from 122 to 196 days. They were divided into three groups of 8 rats each corresponding to the three water temperatures mentioned above. Each of these groups contained 4 males and 4 females, and as far as possible an equal number of litter mates.

The rats were run each evening approximately 5 hours after feeding, for 15 nights, each rat having one run an evening and that always at the same temperature. The order of the runs was irregular. On the first night the animals were given a preliminary run to accustom them to the situation. They were merely dropped into the tank at a distance of 128 cm. from the end with the remaining portion of the trough, which

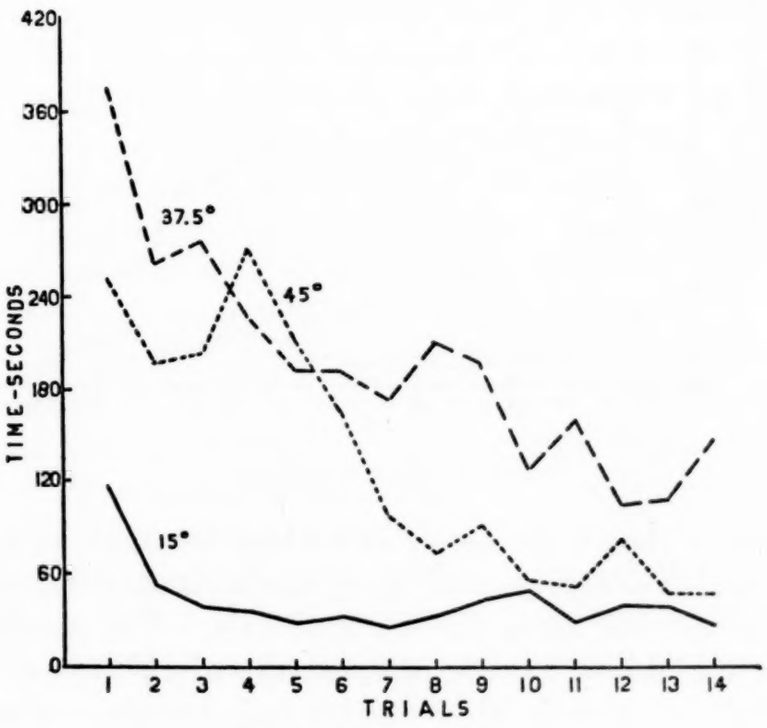


FIG. 2. Mean time scores.

included the alleys, blocked off. No time records were taken for this run.

The results for the succeeding runs, during which the regular procedure was followed, are shown in Fig. 2. Trials are shown on the horizontal axis, and the time in seconds on the vertical axis. Each point on the curves represents the mean time of a group of 8 rats.

From the curves it can be seen that the lowest time scores were at the lowest temperature, 15°. The curve for that temperature shows that there was a comparatively large drop in time within the first 5 trials and that from that point the curve was fairly straight and regular. With the temperature of 37.5° there was a great increase in the time scores. This curve is very uneven and shows that the learning was both slow and irregular. At 45° the time scores were large for the first 5 trials as compared with those of the 15° group, but thereafter they fell rapidly until ultimately they approximated very closely the scores for that group.

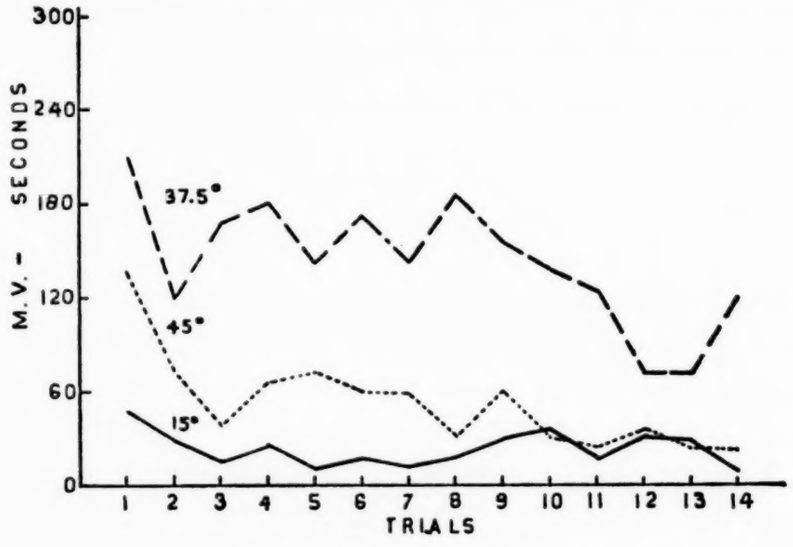


FIG. 3. Mean variations.

Figure 3 shows the mean variations for each group. As before, trials are represented along the horizontal axis and the M.V.s in seconds along the vertical axis. The results show that the variability for the temperature of 15° was relatively slight and the curve is fairly regular and straight. The curve for the temperature of 37.5° showed the greatest variability,

and is highly irregular and erratic. This temperature is about the average body temperature in the rat. At the temperature of 45° the variation for the first 3 trials was considerable, but, as learning progressed, the variation became less, the curve very closely approximating that for 15°.

The three different temperatures of water produced markedly different types of behavior. At the lowest temperature of 15° the rats, when first immersed, reacted to the stimulation of the cold water by strenuous movements. Hesitation was shown at the beginning, but after a few trials, the rats struck out in a direct path toward the platform at the end. Avoidance of the blind alleys was learned fairly early, and errors thereafter were few in number. With the temperature at 37.5°, the rat when immersed almost never showed signs of vigorous activity, but rather displayed a listless or inquisitive attitude. There was no sign of any compelling incentive to activity. The swimming movements were slow. The blind alleys were avoided only after several trials as compared with the rats in the cold water. There were frequent reversals and re-entrances into the blinds. Even after the correct paths were learned, these rats frequently interrupted their running by clinging to the end of an alley. At 45° the movements were more rapid than those for 37.5°, but had a jerky, spasmodic character with frequent diving and vain attempts to climb out over the sides. The behavior had the general aspects of confusion. The correct pathway however was learned fairly quickly, but the rats did not eliminate errors as soon as the 15° group.

The results of this experiment show that the learning of a simple water maze is in part a function of water temperature. Learning was more rapid and more regular for a temperature of 15°. At a temperature of 45° the behavior at first was marked by confusion and progress was slow, but later a fairly high degree of proficiency was gained. When the temperature was 37.5°, which is average body temperature for the rat, progress was slow and highly irregular, and, within the limits of this experiment, the rats failed to attain the level of proficiency shown by the other two groups.

(Manuscript received February 15, 1933)

A TECHNIQUE TO CONTROL, AND TO MEASURE THE EFFECTS OF, FIXATION IN THE 'RANGE OF ATTENTION' EXPERIMENT

BY H. R. CROSLAND

University of Oregon

The weakest aspect of many investigations employing tachistoscopic exposures, including my work described in two earlier papers,[1] consists of a failure to control the fixation of the subject's eyes or to offer some means of knowing just where fixation actually occurred, in case it failed to obey instructions. The technique presently to be described is one which, judging by recent results (to be published elsewhere), "fills the bill" and is worthy of supplanting those devices which I have previously described. To comprehend the newer technique, it will not be amiss to recapitulate the features which characterized the older (and apparently the less efficient) method. The former devices consisted of:

Instructions urging central fixation
Reiteration of instructions at each sitting
Practice exposures, prior to main exposures
Short, average, and long series, ranging from 3 to 10 letters
Random or chance orders of the presentation of 3 to 10 letter cards
Exposure periods of 100 or 150 σ

Of these only the last three were competent to force the subject to approximate the conditions called for in the instructions. And the last one possessed no greater utility than perhaps to prevent re-fixations during the exposure.

Looking over this list one is quite entitled to question their efficacy. These devices are certainly not of a sort to

[1] The Range of Apprehension as Affected by Inter-Letter Hair-Spacing and by the Characteristics of Individual Letters, *Jour. Applied Psychol.*, 1928, 12, 82–124; and Letter-Position Effects, in the Range of Attention Experiment, as Affected by the Number of Letters in Each Exposure, Jour. Exp. Psychol., 1931, 14, 477–507.

answer the question: how was it known that these devices fulfilled or failed in their mission? In fact, other investigators [2] have asserted that, with fixation maintained in the center of the field (where I had hoped it was in my experiments), results varying from mine are to be obtained. Consequently, it may be that the above devices have not wholly lived up to expectations.

To test the influence of fixation (and to carry through certain other plans), we have prosecuted an investigation on 120 subjects with as many as 3,240 main exposures; and the work is being continued on 100 additional subjects. And, while it is not the purpose here to present the results, I wish to describe a technique which I believe has proven its capability beyond question in not only controlling fixation but in measuring its effects when it varies.

The first (1) aspect of the newer technique takes its origin from a feature of the older technique, namely the short letter-series, of 3 letters, which was designed to cause the subject to fixate blank cardboard in case he failed to fixate the center of the exposure-field. In the present set-up, every card carries nine, 14-point, lower-case Caslon Bold letters, in nonsense arrangement. One third of the cards, however, carry letters printed to the left of the center of the card, with the 9th or rightmost letter in exactly the center. These are called (*a*) *Left-Prints*. Another third of the (27) cards carry letters printed exactly in the center, with the 5th or middlemost letter at center. These represent (*b*) *Center-Prints*. And the remaining third of the cards contain letters printed to the right of center, with the 1st or leftmost letter just at center. These are (*c*) *Right-Prints*. The entire scheme is called (1) *Method of Print*. The object of this plan is obvious: should the subject not 'play the game,' by fixating the center of the

[2] Particularly M. A. Tinker: The Effect of Color on Visual Apprehension and Perception, *Genet. Psychol. Monogs.*, 1932, 11, No. 2, 61–136. See pages 77 to 84. Another reason is, however, proposed by Tinker for our discordant results, namely my failure to avoid short series. In this he is mistaken, as I have shown in the second reference in footnote 1, previous page.

Other investigators, at the University of California at Berkeley, claim to have discovered, in unpublished results, a curve the opposite to mine in form when fixation is determined at the right side of the exposure field.

field as instructed to do, he will be caught many times staring at blank cardboard during the exposure.

But to force him to conform to instructions, another expedient is adopted. The above-mentioned 27 cards are presented to him in a random or chance order (2). Under such conditions, he quickly learns not to be too expectant of a given style of *print*. Also (3) some 200 preliminary trials, before the main experiment begins (with a new set of cards), further increase his attitudes and habits of fixating the center of the field, as his most hopeful means of catching whatever *print* confronts his gaze.

A complementary plan to (1) above (and having to do with a wholly different purpose of the investigation [3]) has been to confuse and confound the subject further by requiring him, after exposures, to 'report' in writing, the letters 'recalled,' in *various orders of reproduction* (4). No longer was he allowed to employ alone his older and accustomed left-to-right method of recording the 'recalled' letters.

A random, chance sequence of such *varying-'report'-orders* represented scheme (5) for obtaining his full cooperation as regards central fixation. Not knowing before or during an exposure what would be demanded as to *order of 'report,'* by paper scoring blank, just afterward, he had additional motives of fixating the center of the field as his best expedient for a high score.

The 200 practice trials, prior to the experiment proper, described above as expedient (3), served also to habituate the subject not only to the chance sequence in which he would be asked to reproduce his 'recalled' letters but to the unusual methods of reproduction to which he had not, hitherto, been accustomed. In fact, the very frequency of the practice trials was designed mainly for the latter purpose.

Method (6) consisted of the obtaining, in written form in blanks in the lower half of the 'report' sheet, of introspective

[3] I do not wish to discuss this technique in detail here, for another paper, elsewhere, accomplishes this. Moreover, subsequent papers will offer statistical and graphic proof not only of the effectiveness of this whole technique but of this complementary device also. My only purpose now is to make accessible to other investigators a set-up which has fulfilled its mission of measuring the effects of fixation, as well as controlling it.

answers to eight pertinent questions concerning 'recall' as against written 'report' of the letters seen just previously. A sample of the lower half [4] of the record or 'report' sheet is shown below.

LOWER HALF OF RECORD SHEET:
Introspective Data:

1. Where was your fixation directed *just prior* to the moment when the exposure occurred: ———————————————

2. At the moment the exposure began where was your fixation directed:———————
———————————————

3. Did fixation move or alter its position after the exposure occurred, that is, during the duration of the exposure itself:———. If so, how many times did it so alter or change:———. If it altered, from what point to what point or points did it move, so far as you can now recall:———————
———————————————

4. Did *any* of the letters come to your mind, during the writing, in a different order than the one you were required to follow in writing:———. If the RECALL order differed from that of the WRITING, please state in detail the order of the RECALL:
———————————————

5. Did any letters occur to you, during the writing itself, which you could *not* recognize as having been seen during the exposure:———. If so, what in detail were such letters:———————————————

A hair-line cross (x) was presented in the exact center of the exposure field (7). Suitable tachistoscopic and timing devices permitted its presentation, and the fixation of it by the subject, just prior to a main exposure. They also permitted the pre-exposure, of this cross, to pass over into main exposure without a break in the illumination of the tachistoscope chambers and without the x superimposing itself upon any of the main exposure letters. When the main exposure ended, the cross reappeared and persisted till the experimenter shut it off by a switch.

Typed instructions (8) in the early stages of the practice periods stressed the necessity of the subject's always fixating the center of the exposure field. Just before every single exposure, in practice as well as main series, some portion of these instructions was repeated to the subject. In fact, the experimenter delayed or refused to make the main exposure until the subject signified that he had the center x fixated. At that moment, a 'Ready!' signal ushered in the transforma-

[4] No useful purpose can be accomplished here by showing the entire record blank. Other papers will disclose it, as well as the full data on this and many other questions.

tion of the pre- to the main-exposure, the latter presenting a card with its nine letters. In case subsequent records, objective or introspective, revealed that fixation had not been located as directed, the records were thrown away, and the card was re-presented later in the random sequence. Such failures to fixate properly occurred only eight times in 3,240 presentations.

And, finally, no such technique could hope to succeed unless the brevity of the exposure period (9) offered some guarantee that eye-movements would not occur during the exposure. Hence, the accepted period of exposure of 100 σ was employed. The literature is almost one in asserting that a period of this brief duration will not allow eye-movements during the presentation.

All in all, the above set-up has proven its effectiveness: it controls fixation and it measures the effects of fixation wherever the latter happens to be directed. Convincing evidence in support of these claims the reader will find shortly in other periodicals.

(Manuscript received February 3, 1933)

AN IMPROVED TECHNIQUE IN THE MIRROR-TRACING EXPERIMENT

BY C. E. LAUTERBACH

West Virginia Wesleyan College

The tracing of a star, or other figure, by its reflection in a mirror has been widely used in the laboratory as an experiment in trial and error learning. Scoring the performance has always offered considerable difficulty. Two scores have been necessary, a time score and an error score. This in itself has been a disadvantage but an additional criticism is found in the fact that it is frequently impossible to determine accurately just what constitutes an error.

The illustration (Fig. 1) shows the figure to be traced

FIG. 1. Showing the eleventh trial of a ten-year-old girl (grade 5, I.Q. 108) in the mirror-tracing experiment. The record shows that she made 34 errors in a total number of 132 possible points. Her time score is 1 min 33 sec.

divided into units so that the number of units traced per minute may be computed and a single rate score secured. The total number of units in the star is 132. The requirement is that the line traced by the subject must cut through, or run tangent to, each circle constituting the star. In the illustration the subject has failed to meet this requirement in 34 instances. Her unit score, therefore, is (132 − 34) or 98. Her time score, as secured with a stop watch, is 93 seconds. Her rate per minute becomes (98 ÷ 93) × 60 or 63.2.

By this method a single score is secured and when successive tracings are made a single learning curve may be constructed. Such a curve is shown in Fig. 2, covering 150

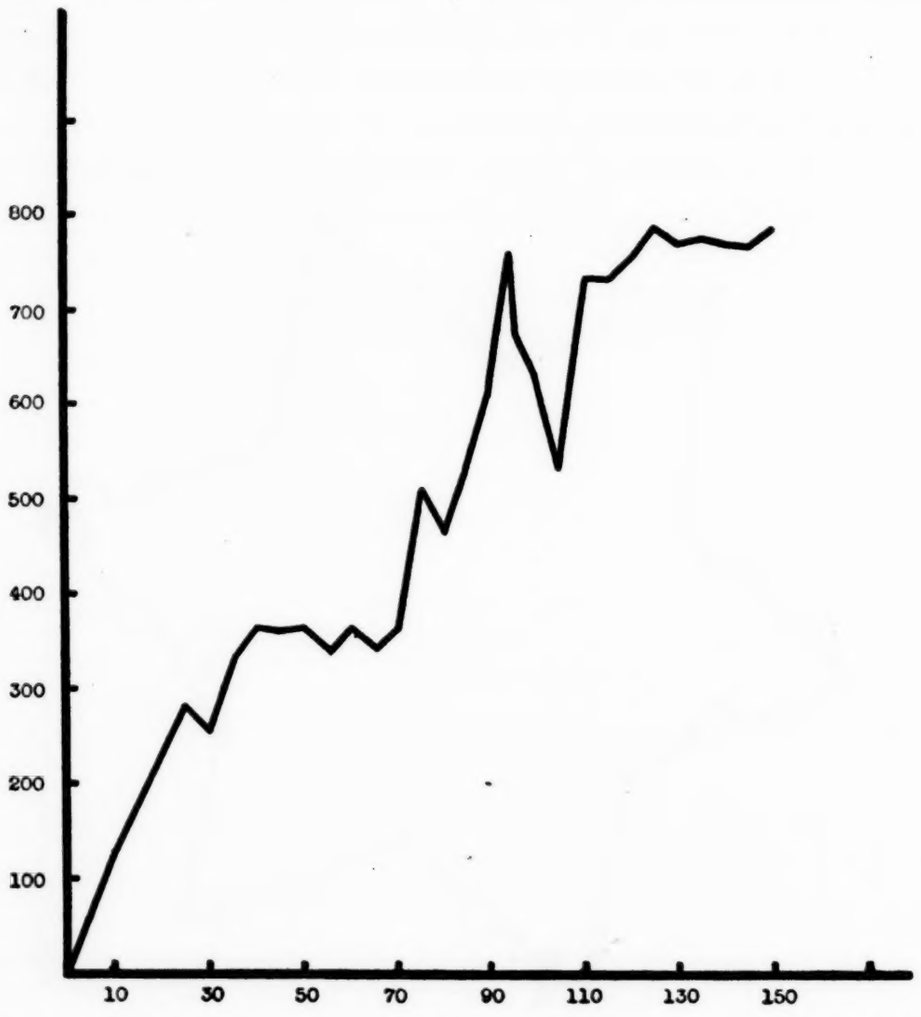


FIG. 2. A learning curve showing the average of each five successive trials in a total of 150 in a morrir-tracing experiment. The horizontal axis represents the number of trials; the vertical, the number of units traced per minute.

trials. The scores in terms of rate per minute are shown in Table I. It is likely that trials 120 to 150 represent the physiological limit for this function.

TABLE I

SHOWING SCORES IN TERMS OF RATE PER MINUTE IN THE MIRROR-TRACING EXPERIMENT

Trial	Score	Trial	Score	Trial	Score	Trial	Score	Trial	Score
1	33.1	32	344.3	10 min. int.		90	609.5	120	789.8
2	57.7	33	312.0	61	314.4	91	687.2	121	792.0
3	73.4	34	390.0	62	344.3	92	680.0	122	786.0
4	97.9	35	340.9	63	341.7	93	681.8	123	790.5
5	83.3	25 min. int.		64	360.0	94	610.0	124	795.0
6	96.2	36	282.2	65	368.3	95	697.1	125	783.5
7	116.4	37	374.2	66	327.5	96	620.8	126	780.0
8	144.4	38	390.0	67	352.0	97	665.4	127	769.3
9	135.7	39	378.0	68	416.8	98	585.0	128	795.0
10	155.7	40	413.6	69	341.7	99	638.1	129	746.6
11	164.6	41	444.6	70	377.1	100	645.0	130	773.3
12	164.6	5 min. int.		71	451.7	8 day int.		131	756.0
13	172.1	42	378.0	72	507.1	101	441.1	132	768.0
14	159.1	43	365.7	73	529.6	102	468.7	133	774.0
15	236.1	44	330.0	74	533.7	103	483.7	134	797.6
16	209.1	45	304.6	75	567.2	104	600.0	135	793.3
17	211.6	46	344.3	45 min. int.		105	683.6	136	780.0
18	254.0	47	360.0	76	358.0	106	709.1	137	764.2
19	254.4	48	341.7	77	452.5	107	774.0	138	774.0
20	260.6	49	385.7	78	488.0	108	720.0	139	770.5
21	292.8	50	393.0	79	548.5	109	762.8	140	780.0
22	275.7	5 day int.		80	483.7	110	714.5	141	764.2
23	336.5	51	291.1	81	458.8	111	754.2	142	780.0
24	315.0	52	322.5	82	524.0	112	714.5	143	771.4
4 day int.		53	327.5	83	529.6	113	780.0	144	756.0
25	178.6	54	374.3	84	552.8	114	720.0	145	770.5
26	234.5	55	386.3	85	595.3	115	720.0	146	806.6
27	258.0	56	354.0	86	650.0	116	720.0	147	793.3
28	260.6	57	339.1	87	674.0	117	754.2	148	773.3
29	241.9	58	374.3	3 min. int.		118	762.0	149	794.7
30	284.4	59	374.3	88	512.0	119	770.5	150	804.7
31	286.6	60	371.4	89	640.0				

(Manuscript received December 21, 1932)

DISCUSSION

MEMORY FOR PLEASANT, UNPLEASANT, AND INDIFFERENT PAIRS OF WORDS

In a recent experiment (1) the subjects were instructed to classify words into three groups—pleasant (P), unpleasant (U), and indifferent (I)—according to whether the word or its immediate associations were P, U, or I. The three kinds of words were then arranged in pairs: P–U, U–P, P–I, I–P, U–I, and I–U. The paired words were presented to the respective subjects for learning and later the first member of each pair was given in a retention test. Balancing of the series and other ordinary precautions were observed. After presenting results the writers state: "The most plausible conclusion seems to be that there is little, if any, difference in the relative efficiency of learning and retaining P–U, U–P, P–I, I–P, U–I, and I–U pairs of words."

This study is so typical of work which has been appearing upon the relation between 'feeling' and 'memory' and the so-called 'law of effect' that a brief comment upon methodology is in order.

When words are presented to a subject for classification in terms of P, U, I there is no guarantee that reading a word will evoke a genuine affective reaction. In the present experiment the total setting was such as to make the arousal of an intense affective reaction unlikely. The use of words, the instruction to make a classification into three groups, the learning and later recalling, are not conducive to the development of felt experience. In any case no steps were taken to check the affective reaction either through report of felt experience or through physiological technique.

Words can be readily classified as P, U, I. A recent test of this proves the point (2). But there is no reason to assume that the affective judgment of a word has a univocal relation to felt experience. The word 'cheerful,' for example, would probably be classified as P merely as a matter of coldly cognitive meaning. There is no reason to assume that the word arouses felt pleasantness.

I have come to the conclusion that failure to distinguish between (1) *felt experience* in which the affective processes have an appreciable intensity and duration, and (2) the cold cognitive *meanings of pleasant and unpleasant*, lies at the root of much confusion within

affective psychology to-day. A good many statements which are correct for (1) are false for (2) and vice versa. This was the main point in a criticism of Wohlgemuth (3).

The pairing of words for learning still further complicates the situation affectively. Is the implication that affective processes are thus paired? With no report upon affective experience we will never know what did occur. Incidentally, it is interesting that the pairs P–P and U–U were omitted.

If one is experimenting upon felt pleasantness and unpleasantness, let affective methodology be considered and especially the soundness of the evidence that genuine felt experience is a factor in the experiential situation. No valid conclusion can be drawn from the experiment in question which concerns *felt* pleasantness and unpleasantness in relation to learning and retention.

PAUL THOMAS YOUNG, *University of Illinois.*

REFERENCES

1. CASON, H. AND F. C. LUNGREN, Memory for pleasant, unpleasant, and indifferent pairs of words, *J. Exper. Psychol.*, 1932, 15, 728–32.
2. YOUNG, P. T., The relation of bright and dull pressure to affectivity, *Amer. J. Psychol.*, 1932, 44, 780–84.
3. YOUNG, P. T., The coexistence and localization of feelings, *Brit. J. Psychol.*, 1925, 15, 356–63.

(Manuscript received December 20, 1932)

METHODS OF STUDYING THE LEARNING AND RETENTION OF PLEASANT AND UNPLEASANT ACTIVITIES.

BY HULSEY CASON
University of Wisconsin

Professor P. T. Young (1) has taken a 4-page experimental paper by Cason and Lungren (7) as the point of departure for a critical comment on the methods of studying the learning and retention of pleasant and unpleasant activities. As a result of the policy of the JOURNAL and the kindness of the Editor, I have been allowed to see Professor Young's paper before its publication, and I am also permitted to make a reply. I have already discussed the questions raised by Professor Young in

other connections, and the present answer can therefore be brief.

Professor Young questions the statement made by Cason and Lungren (7) that "The most plausible conclusion seems to be that there is little, if any, difference in the relative efficiency of learning and retaining P–U, U–P, P–I, I–P, U–I, and I–U pairs of words." This result is closely related to two conclusions which the present writer reached in a monograph on 'The Learning and Retention of Pleasant and Unpleasant Activities' (3); first, that "There is little, if any, difference in the efficiency with which P and U activities can be learned" (3, 75), and second, that "There is little, if any, difference in the efficiency with which activities that were P and U when they originally occurred can be retained and reproduced" (3, 77). In the monograph (3) I have summarized the experimental evidence which supports and contradicts each of these conclusions (pp. 75–80), and I have also described the methods which have been employed by all who have worked in this field (pp. 11–29). I have also dealt with the question of method in three other publications in 1932 (4, 5, 6). Perhaps I should have made better use of Professor Young's valuable studies in affective psychology, but I was not aware that any one of them was especially concerned with the learning and retention of pleasant and unpleasant activities.

In the short experiment (7) to which Professor Young refers, the subject classified each word as P, U, or I "if the word itself was P, U, or I to him, or if the immediate associations were P, U, or I." After each word had been classified in this way it was referred to as a P, U, or I word. Professor Young says that "When words are presented to a subject for classification in terms of P, U, I there is no guarantee that reading a word will evoke a genuine affective reaction." In the article in question (7) we did not say anything about a 'genuine affective reaction,' and I do not know the exact meaning that Professor Young would attach to this expression. My own views on the general nature of affectivities have been set forth in two theoretical papers in 1930 (2) and in 1933 (9).

Professor Young says that "In the present experiment the

total setting was such as to make the arousal of an intense affective reaction unlikely," but we did not claim that the feelings of our subjects were intense. I have even argued elsewhere that the mild affectivities are in special need of further investigation. There are several apparently good reasons why words are important as learning material; and the relation between the learning and retention of words, on the one hand, and on the other the mild feelings connected with the words is a problem that is worth investigating. A further study of this subject by Silverman and Cason (**10**) is in press.

In the experiment in question (**7**), each subject classified the words as P, U, or I, and the words were then arranged in pairs and used as learning and retention material. If a word was classified by the subject as a P word at the beginning of the experiment it was considered a P word during the rest of the experiment. Because of the nature of the experiment, no check could be made of whether or not an affectivity continued to be evoked by each individual word. I agree completely with Professor Young that it is frequently desirable to determine by means of the verbal report method whether or not an affectivity is actually present, and I have expressed some belief in this proposition in several places (**3, 4, 5, 6, 8, 9**). In the present study (**7**), however, frequent verbal reports would have altered the nature and purpose of the experiment. Taking the subject's introspections every time he looked at each word, every time he thought about each word, every time he imagined each word, every time he spoke each word, every time the experimenter spoke each word, besides being a practical impossibility, was also unnecessary. The single conclusion that was drawn from the short experiment in question (**7**) was based on the combined results of 50 subjects. At the beginning of the experiment, these subjects classified 2336 words as P, 2336 words as U, and 2336 words as I. It is of course highly improbable that each of these words continued to be definitely P, U, or I at all times during the experiment. But the 2336 P words were on the average more P than the 2336 I words, and the 2336 U words were on the average more U than the 2336 I words. This is the assumption on which

the single conclusion that was drawn depends, and in the experiments in this field it has not been customary to labor this point. Affective psychology has not yet reached the stage where accurate quantitative measures are possible, but this does not mean that qualitative studies should be discouraged.

Professor Young says that "There is no reason to assume that the affective judgment of a word has a univocal relation to felt experience." We did not assume this. He says the word 'cheerful' would probably be classified as P; but we did not make this assumption either. Our subjects classified the words according to their own personal reactions, the same words were classified differently by different subjects, and the subjects were instructed to classify a word in the indifferent class if it was indifferent to them. Professor Young says that "The pairing of words for learning still further complicates the situation affectively. Is the implication that affective processes are thus paired?" We fully realized that the psychological activities involved in the experimental situation were not simple, and we neither thought nor said that the affective processes were paired. Professor Young writes further that "Incidentally, it is interesting that the pairs P–P and U–U were omitted." The reason for omitting the P–P and U–U pairs of words in the article in question (**7**) was that the present writer had previously published two experiments on the relative efficiency of learning and retaining P–P, U–U, and I–I pairs of words (**3**, 39–50). A reference was also made to these earlier studies on the first page of the paper that Professor Young is discussing (**7**, 728).

Finally, Professor Young stresses the distinction between felt experience and the cold cognitive meanings of pleasant and unpleasant, he believes that affective methodology should be considered when one is experimenting upon felt pleasantness and unpleasantness, and he says that genuine felt experience is a factor in the experimental situation. I agree with him heartily on all of these points, and I have already discussed them in a theoretical paper (**9**) which is to appear in the July, 1933, number of the *Psychological Review*. I have not

minimized the role of the conscious and verbal activities which are involved in the affectivities. But are we to understand that there is only one 'affective methodology'? If so, whose 'methodology'? I have not been able to avoid the impression that the genetic approach to these subjects is important, and that the learning, retention, and reproduction of the feelings and emotions themselves are worthy of consideration.

REFERENCES

1. YOUNG, P. T., Memory for pleasant, unpleasant, and indifferent pairs of words, *J. Exper. Psychol.*, 1933, **16**, 454–455.
2. CASON, H., Pleasant and unpleasant feelings, *Psychol. Rev.*, 1930, **37**, 228–240.
3. CASON, H., The learning and retention of pleasant and unpleasant activities, *Archives of Psychology*, 1932, **21**, No. 134, pp. 96.
4. CASON, H., The pleasure-pain theory of learning, *Psychol. Rev.*, 1932, **39**, 440–466.
5. TROWBRIDGE, M. H., AND CASON, H., An experimental study of Thorndike's theory of learning, *J. Gen. Psychol.*, 1932, **7**, 245–260.
6. CASON, H., Review of E. L. Thorndike's 'Human Learning' (1931), *J. Abn. and Soc. Psychol.*, 1932, **27**, 214–222.
7. CASON, H., AND LUNGREN, F. C., Memory for pleasant, unpleasant, and indifferent pairs of words, *J. Exper. Psychol.*, 1932, **15**, 728–732.
8. CASON, H., AND CASON, E. B., Affectivity in relation to breathing and gross bodily movement, *J. Gen. Psychol.*, 1933, **8**, in press.
9. CASON, H., An interacting-pattern theory of the affectivities, *Psychol. Rev.*, 1933, **40**, in press.
10. SILVERMAN, A., AND CASON, H., Incidental memory for pleasant, unpleasant, and indifferent words, *Amer. J. Psychol.*, 1934, **45**, in press.

(Manuscript received January 10, 1933)

A NOTE ON TOLMAN'S 'DISPROOF' OF THORNDIKE'S LAW OF EFFECT

BY FLORENCE L. GOODENOUGH
University of Minnesota

In the December, 1932, number of this JOURNAL there appeared an article by Tolman *et al*[1] presenting certain experimental data interpreted by the authors as contradictory to Thorndike's Law of Effect. Before accepting these conclusions at their face value, it is well to make a critical

[1] Tolman, E. C., Hall, C. S. and Bretnall, E. P., A disproof of the law of effect and a substitution of the laws of emphasis, motivation and disruption, this JOURNAL, 1932, **15**, 601–614.

examination of the evidence presented to see whether other interpretations are possible. As a result of such an examination I suggest the following as alternative conclusions:

(*a*) The differences found may be attributable to differences in the learning ability of the several groups rather than to the differences in the methods employed for learning, (*b*) when allowances are made for these original differences, no one group shows a reliable superiority over another but such small differences as are found take the direction to be expected on the basis of the 'Law of Effect,' (*c*) the conditions of the experiment are not suitable to test the hypothesis under consideration.

(*a*) All groups were given a fore-exercise consisting of three trials without specific instructions but designed to acquaint them with the set-up of the board. During this fore-exercise (which was uniform for all groups) considerable opportunity for learning was afforded. The differences between the groups at the outset of the experiment proper cannot, therefore, be fairly attributed to chance but rather to unequal learning ability of the several groups as shown by their relative performance when learning conditions were the same for all. The assumption that these differences are not the result of chance is given further support by the consistency with which they are maintained throughout the early part of the experiment (*op. cit.*, Table 2).

(*b*) As learning proceeds, the original differences among the four groups steadily decrease. This is not what one would expect to find if the differences were the *result* of the experimental conditions set for learning, but is more in line with the hypothesis that the most able groups were obliged to adopt the least effective methods of learning, and vice versa.

Furthermore, if one takes as a measure of learning, not the total number of errors made on all trials regardless of their position in the series but the percent that the number of errors made by each group on the twentieth trial is of the number made on the first experimental trial (that is, at the end of the period of uniform practice given as a fore-exercise) one obtains the following figures: The Bell-Right group has reduced its

errors to 8.9 percent of its original average. The corresponding figures for the other groups are, Bell-Wrong, 10.3 percent; Bell-Shock-Wrong, 9.8 percent; Bell-Shock-Right, 10.3 percent.

These figures would lead to a conclusion exactly opposite from that of the authors which they express as follows (op. cit., pp. 605–606):

"It appears at once that the Bell-Right and the Bell-Shock-Right curves are better (*not worse*) than the Bell-Wrong and the Bell-Shock-Wrong curves. It also appears that the difference between the Bell-Shock-Right curve and the Bell-Shock-Wrong curve is greater than that between the simple Bell-Right curve and the simple Bell-Wrong curve. And, be it noted, all these facts are contrary to expectation according to any ordinary formulation of the Law of Effect."

The foregoing statement is based upon inspection of the learning curves obtained when the data are 'smoothed' by the cumulative error method. This method, however, is only suitable for use in the study of differences between groups whose learning is done under similar conditions. It makes no distinction between differences existing previous to learning and those appearing later. Unless the groups are rigidly equalized for learning ability at the start, differences due to specific training are likely to be completely obscured by the use of this method.

(*c*) Most of us will agree with Tolman that both bell and shock play the part of *signals* or *emphasizers* to the subject. But this statement in no way conflicts with the Law of Effect, for a signal may be either satisfying or annoying and either a pleasant or an unpleasant experience may be given additional emphasis by the addition of some further element to the situation.

The authors state (p. 605), that "the subject . . . was told to correct himself when he made an error by placing the stylus in the right hole of the pair." Now it is at least an open question whether an unexpected delay of a few seconds in receiving a punishment that is known to be inevitable and which one has braced oneself to receive will be, to the average

normal adult, a more satisfying experience than taking the punishment at once and 'having it over.' In the case of the Bell-Shock-Right group this is what happens. If the stylus is thrust into the right hole of the pair on the first trial, the subject receives a shock and is free to move on to the next pair with the consciousness that he is so much nearer the end of a disagreeable experience. But if the stylus is first thrust into the wrong hole, the only thing gained would be a brief postponement of the punishment, a relief which, to many people, would probably be more than offset by the unpleasant necessity of having to 'get set' all over again. Since both experiences are primarily annoying, and one has a further element of mixed annoyance (at having to repeat the act and receive the alloted punishment) and relief (at not receiving the punishment immediately) it is unsafe to assume that the balance of satisfaction or annoyance rests either with the right or the wrong response. But unless such an assumption is made the entire experiment falls to the ground.

In conclusion it may be noted that when percent of gain is taken as the measure of learning the greatest contrast is that between the Bell-Right and the Bell-Wrong groups, and that this difference is in the same direction as that reported by Thorndike who used the words 'Right' and 'Wrong' in place of the bell as signal. The complicating factors discussed in the last paragraph may perhaps explain why the differences between the Bell-Shock-Right group and the Bell-Shock-Wrong group are so small, but such as they are, they not only fail to disprove the Law of Effect but actually afford some slight support for it.

(Manuscript received December 15, 1932)

THE LAW OF EFFECT: A REPLY TO DR. GOODENOUGH

BY EDWARD C. TOLMAN
University of California

In Dr. Goodenough's note (**4**) on the article by C. S. Hall, E. P. Bretnall and myself (**8**) in which we sought to throw some experimental doubt on the orthodox Law of Effect she makes two main criticisms. She suggests: (1) that the greater inferiority of our Bell-Shock-Wrong group over our simple Bell-Wrong group may be explained as a difference between the learning abilities of the two groups rather than as a true difference arising out of the two specified conditions of learning; and (2) that, even if in general our results did indicate valid differences resulting from our various conditions, those conditions themselves were not properly chosen to test the hypothesis under consideration. I will attempt to reply to each of these criticisms as best I may.

(1) *The Poorer Performance of the Bell-Shock-Wrong Group over That of the Bell-Wrong Group was Due Merely to Sampling.* —Dr. Goodenough points out that even on the first regular trial the Bell-Shock-Wrong group gave a poorer performance than the Bell-Wrong group and she suggests that this indicates that the Bell-Shock-Wrong group had been learning less from the three preceding fore-exercise trials than had the Bell-Wrong group. And from this last she concludes further that the Bell-Shock-Wrong group were, as such, poorer learners than the Bell-Wrong group. Now I am perfectly willing to admit that the sizes of our groups were too small absolutely to prove our point. Undoubtedly before the issue can be finally settled bigger groups must be run. It seems to me, however, that in her choice of the evidence which she brings forward to prove her point she was unfortunate. She says that in the three fore-exercise trials all groups were *treated alike*. Hence, if at the end of them, *i.e.*, on the first regular trial, the Bell-Shock-Wrong group did worse than the Bell-Wrong group, this can be explained only by assuming an innately poorer learning ability on the part of the former group. But it is

just here that Dr. Goodenough seems to have misunderstood our conditions. Our different groups were *not* treated alike during the fore-exercise (vide **8**, p. 604). In the three fore-exercise trials the Bell-Shock-Wrong group, although they did not yet know whether they were later to try to punch, or to try to avoid, the ringing and shocking holes, did nevertheless experience both bell and shock on that one of the two holes in each pair which they were later to try to avoid. The Bell-Wrong group, on the other hand, experienced merely the bell on these same holes.

If, therefore, the Law of Effect in its usual sense had actually operated, then the 'annoyance' of the shock coming immediately after the one hole of each pair should, in the case of the Bell-Shock-Wrong group, have already during the fore-exercise begun to stamp out what were later to be the wrong S-R 'connections.' And hence on the first regular trial the Bell-Shock-Wrong group should have made a better score (fewer errors) than the simple Bell-Wrong group. If, on the other hand, the addition of the shock to the bell instead of stamping out a 'connection' tended merely to lend an additional positively-attracting emphasis or accent to these wrong holes, then during the fore-exercise the addition of this shock should already have begun to make the avoidance of the wrong holes difficult. And on this latter assumption the Bell-Shock-Wrong group should by the time of the first regular trial already have been handicapped and should have started out worse than the Bell-Wrong group. And it was this latter which actually proved to be the case. In other words, the conditions which according to our assumptions served to make the Bell-Shock-Wrong group do poorer than the Bell-Wrong group during the regular trials were already operative in the period of the fore-exercise. And this, I assert, will explain the initial greater poorness of the Bell-Shock-Wrong group just as readily as will Dr. Goodenough's assumption of an innate poorer ability.

Finally, let me again emphasize that I am not completely denying that, given our relatively small groups, Dr. Goodenough's explanation may be the correct one. I am merely

pointing out that the supposed evidence which she cites to support her interpretation, when properly analysed, fails to do so. If anything, it turns against her and adds further fuel to our own previous interpretation.

(2) *The Conditions of the Experiment Were not Really Such as to Test the Law of Effect.*—This, her second main contention, is based primarily upon a consideration of the conditions for the Bell-Shock-Right group. When this group hit the wrong (unshocked) hole first, then they had, she says, the added 'annoyance' of having to brace themselves again ('get set again') for the subsequent shock which they were required to take in the 'correct' hole before they could pass on to the next pair of holes. This added annoyance, so she says, confused the picture and made the situation unsuitable for testing the Law of Effect. Let us accept her interpretation of such an added annoyance and push the analysis further. And let us see what we have in the way of resulting total 'annoyances' and 'satisfactions' for all four groups. If we represent an annoyance by a − sign and a satisfier by a + sign, we then have the following outcome:

Bell-Shock-Wrong Group

Wrong Response − (annoyance of wrongness) — (annoyance of shock) Right Response + (satisfaction of rightness)

Bell-Wrong Group

Wrong Response — (annoyance of wrongness) Right Response + (satisfaction of rightness)

Bell-Right Group

Wrong Response − (annoyance of wrongness) Right Response + (satisfaction of rightness)

Bell-Shock-Right Group

Wrong Response − (annoyance of wrongness) − (annoyance of having to get 'set again') Right Response − (annoyance of shock) + (satisfaction of rightness)

Assuming the Law of Effect, it appears from the above schema that the Bell-Shock-Wrong group should have learned

most readily and that the Bell-Wrong and Bell-Right groups should have been about equal to each other but that they should not have done as well as the Bell-Shock-Wrong group. Just what should have been the fate of the Bell-Shock-Right group is not so clear. But, if we assume that the one annoyance on the Right Response balances one of the two annoyances on the Wrong Response, we should then have left a situation similar to that for the Bell-Wrong and the Bell-Right groups. Or, to sum it all up more briefly, we would have the following:

According to the Law of Effect

BSR, BR, BW } (Equal to each other) ..... Most Errors

BSW ..... Least Errors

But note the difference between the above and the way it did actually come out.

As it actually came out (See 8, Fig. 2)

BSW ..... Most Errors
BW ..... Next
BSR, BR ..... Least Errors

Comparing these two schema we note that it may be, as Dr. Goodenough suggests, that the reason the BSR group came out no worse than the BR group is to be explained by assuming the Law of Effect. For that one comparison the expectations according to the Law of Effect and the actually obtained facts do agree. But since for all the other possible comparisons the assumptions of the Law of Effect and the actually obtained facts diverge most arrantly, I for one feel no particular urge to assume the truth of the Law of Effect in that one case.

In Conclusion.—It may perhaps be desirable if I now state more positively my own doctrine. The subject in learning a pair of holes in such a situation is not, I would contend, 'stamping in' one S-R 'connection' and 'stamping out' another S-R 'connection.' Rather he is *learning* (making

associations to the effect) that one of the holes in the pair will, if punched, lead to a designation of 'correctness' and the other to a designation of 'incorrectness.' It is to be noted that the different groups were in this experiment presented with different types of such designations. For the Bell-Right group, bell, and for the Bell-Wrong group, lack-of-bell designated 'correctness' while *pari passu* lack-of-bell and bell, respectively, designated 'incorrectness.' Similarly for the Bell-Shock-Right group, bell-plus-shock designated correctness and lack-of-bell-plus-shock designated incorrectness; and, vice versa, for the Bell-Shock-Wrong group.

Assuming, now, that the subjects have acquired these associations between the holes and these to-be-expected [1] respective designations of correctness and incorrectness and that in addition the subjects have a motive for wanting to choose the holes with the designation of 'correct,' then the actual performance of these subjects will come to be that of punching the correct holes only and of avoiding the incorrect holes. But the subjects may not have this motive or they may change their motives. And then they will not *perform* in this prescribed fashion, even though they have *learned* the proper associations. *In other words, performance and learning need not necessarily go together.*[2] Thus, for example, once a given set of subjects had learned and were performing correctly in the above experiment, most of them could, I am sure, have then and there reversed their performances practically without error and punched in just the opposite ways. But this according to orthodox notions would have meant that they had suddenly reversed their S-R 'connections' *without any further learning.* But that would have been nonsense.

Neither 'success,' nor 'failure,' nor the word 'right,' nor the word 'wrong,' nor 'shock,' nor 'lack-of-shock,' nor 'bell,' nor 'lack-of-bell' stamp in or stamp out. They simply 'result.' And *learning* consists in the subject's coming to be properly set for—coming properly to *expect*—such results.

[1] N.B. *Such expectations are by no means necessarily conscious.*

[2] This need to distinguish between learning and performance was probably first conceived by M. H. Elliott (3). It has also been noted by Lashley (6). See also Tolman (7), 363 f.

Performance on the other hand, consists in then *utilizing* appropriately these learned and thus to-be-expected results—given the motivation conditions of the moment.

What is ordinarily called just 'learning' would then according to such a view divide into two successive processes or functions: (*i*) *learning proper*, *i.e.*, the acquiring of 'expectations of results'; and (*ii*) the *utilization in performance* of such learned (or being learned) expectations—*i.e.*, a utilization appropriate to the then governing conditions of motivation. And this distinction would mean also that any so-called law of learning would have to be considered further as to whether it really applied to *learning proper* or to *utilization in performance*, or to both.

Let me review, finally, from this point of view the three laws advanced in the original article by Mr. Hall, Miss Bretnall and myself.

The Law of Emphasis.—This law would seem to belong both under the head of (*i*) *learning proper* and under that of (*ii*) the *utilization of learning in performance*. (*i*) The more 'emphatic' the to-be-expected results of either hole the more readily the expectations as to these results for both holes and the differences between these expectations should be acquired. (*ii*) Further, however, it also seems that in so far as that hole, which according to the motivation conditions is the one to be positively responded to, happens also to be the one which receives the greater 'emphasis,' the more easily the learned difference between the two holes can be *utilized in performance*. (Vide: the superiority of Bell-Right over Bell-Wrong.)

The Law of Disruption.—This law would apply primarily only to *learning proper*. If pain results from the experience of either hole, it apparently causes a disruption—*i.e.*, a phenomenon similar to retrograde amnesia.

The Law of Motivation.—This is a law which seems to apply primarily to *utilization in performance*. For, the stronger the motive, the more successfully the individual will utilize what he has learned relatively to the to-be-expected results for the two holes. It may be, however, that this law also applies in some degree to *learning proper*. But to just

what extent it applies to this latter is at present not known. Indeed an examination of the facts of 'latent learning' as it is found in rats (vide: **1**, **2**, **5**, **9**, **10**) suggests little evidence for the law in this latter sense since the rats learned (although they did not perform) just as well without strong motivation as with strong motivation. Nevertheless, it would seem reasonable to suppose *ad hoc* that, if an individual has a strong motivation to utilize his learning, as fast as he acquires such learning, that this motivation will tend in some measure, at least, to hasten not only the correct utilization of what he has learned but also actually to speed up this learning itself.

One Last Word.—It should be pointed out that it seems to have been this Law of Motivation, as it works in *utilization in performance*, which led to the mistaken Law of Effect. An individual who, due to the conditions of the experiment, is strongly motivated to utilize his learning in his performance will, of course, perform more correctly at any given stage of his learning than will an individual who is not thus strongly motivated. But this does not mean that the relative 'successes' and 'failures' of the alternative acts 'stamp in' the one set of acts and 'stamp out' the other. These resultant effects determine merely that the individual shall tend to *perform* the one set of acts and not to perform the other set in so far as he has already *learned* to expect those effects. But this previous *learning* of the to-be-expected results is, as I have argued, itself dependent upon other factors than the 'successfulness' and the 'failingness' per se. *Learning* (as distinct from performance) will be affected by an 'emphatic' or 'disruptive' quality in the effects. And it will of course be affected by a host of other conditions which, however, were not examined in the present experiment. (Among the most important of these other conditions would, I believe, be one leading to a law stating the value of a '*repetition*' of the experiences as the main conditioner of *learning*.)

REFERENCES

1. BLODGETT, H. C., The effect of the introduction of reward upon the maze performance of rats, *Univ. Calif. Publ. Psychol.*, 1929, **4**, 113–134.
2. ELLIOTT, M. H., The effect of appropriateness of reward and of complex incentives on maze performance, *Univ. Calif. Publ. Psychol.*, 1929, **4**, 91–98.

3. ELLIOTT, M. H., Some determining factors in maze performance, *Amer. J. Psychol.*, 1930, **42**, 315–317.
4. GOODENOUGH, F. L., A note on Tolman's 'disproof' of Thorndike's Law of Effect, *J. Exper. Psychol.*, 1933, 459–462.
5. HANEY, G. W., The effect of familiarity on maze performance of albino rats, *Univ. Calif. Publ. Psychol.*, 1931, **4**, 319–333.
6. LASHLEY, K. S., Learning: I. Nervous-mechanisms of learning, *The Foundations of Experimental Psychology* (Worcester, Mass.: *Clark University Press*, 1929), 524–563.
7. TOLMAN, E. C., *Purposive Behavior in Animals and Men* (New York: The Century Co., 1932).
8. TOLMAN, E. C., HALL, C. S., AND BRETNALL, E. P., A disproof of the law of effect and a substitution of the laws of emphasis, motivation and disruption, *J. Exper. Psychol.*, 1932, **15**, 601–614.
9. TOLMAN, E. C., AND HONZIK, C. H., Introduction and removal of reward, and maze performance in rats, *Univ. Calif. Publ. Psychol.*, 1929, **4**, 257–275.
10. WILLIAMS, K. A., The reward value of a conditioned stimulus. *Univ. Calif. Publ. Psychol.*, 1929, **4**, 31–55.

(Manuscript received January 16, 1933)